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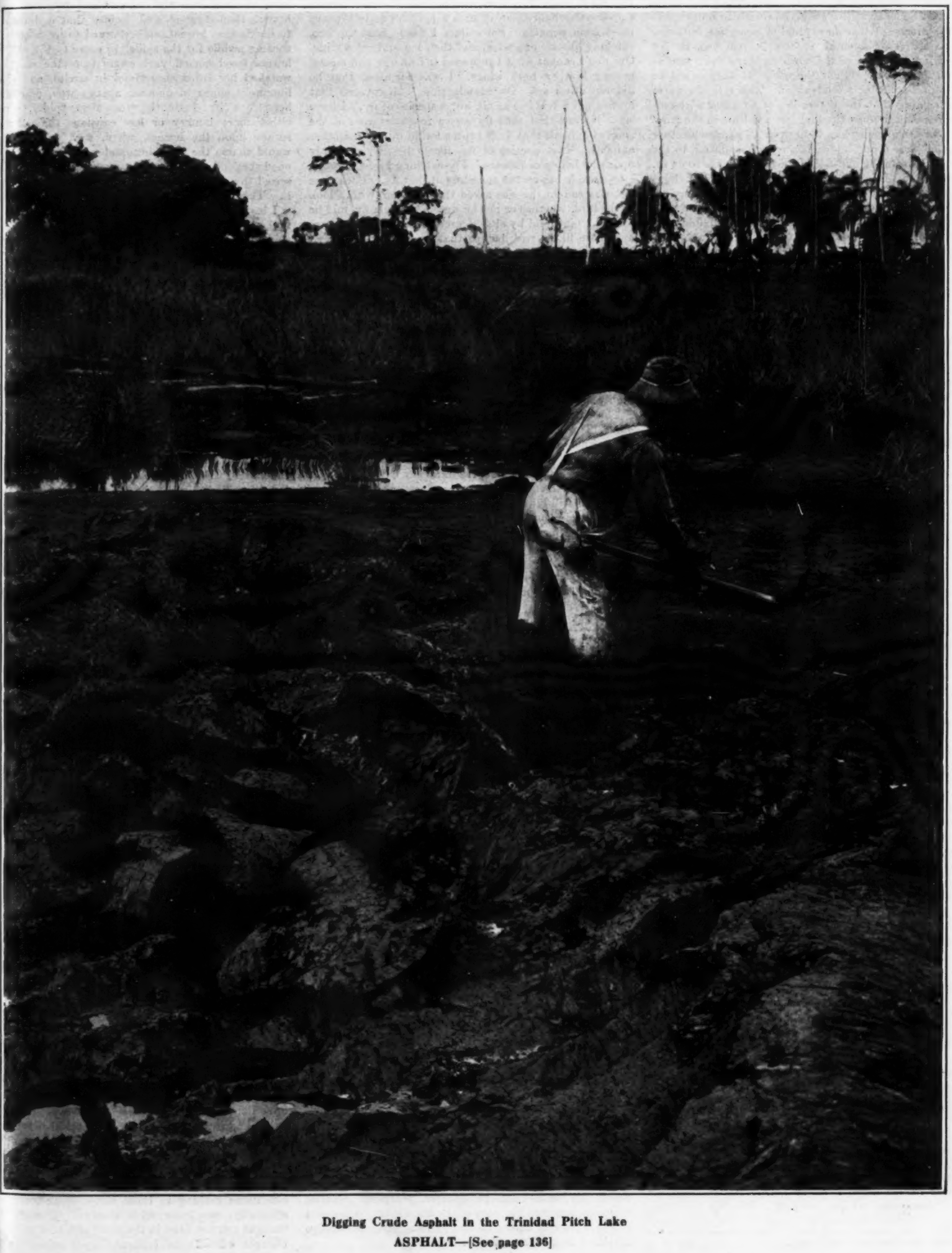


Digging Crude Asphalt in the Trinidad Pitch Lake

ASPHALT—[See page 136]

Mimicry in Animals*

A New Theory of Inherited Experimental Knowledge

By Hudson Maxim

When a bird casts his eye upon a worm, there is question in his glance. He looks inquiringly at the worm, to be assured that it is a proper food-worm, and not some poisonous serpent that he or his ancestry has had trouble with. As he looks at the worm, he recalls to mind previous experiences, either individual or ancestral, with the puff-adder. That mental process is reflected in the mental impressorium of the worm in some such wise as though the bird should ask the worm whether or not he were a puff-adder or a food-worm. This sets the worm to concluding that the possession of so small a resemblance as even to rouse the slightest suspicion in the mind of the bird, and cause him to hesitate to inquire whether or not he were a puff-adder, is a recommendation to the worm to imitate the puff-adder, and he proceeds to do so.

This same telepathic process must exist throughout nature. Of course, the degree in which it exists may be, and doubtless is, infinitely slight. The influence on each individual occasion may be practically negligible, but when this influence has been exerted billions upon billions of times, the cumulative effect is very great.

This is why one form of butterfly chooses to look like a dead leaf, another like a green leaf; why the chameleon changes his color, why the Brazilian butterfly looks like an owl that is the greatest enemy of the bird that is the butterfly's greatest enemy.

The quick glance of the flitting bird at the remote progenitor of the walking-stick beetle, to decide whether or not it were a dry twig, taught the beetle to imitate the dry twig for its protection.

It must be remembered that there is no dividing line between mental and physical processes—the one merges into the other. The mental process is a physical process, and all physical processes are to a large extent mental processes.

The very fact that we are so highly organized places us out of sense with many things with which we would be keenly in sense were we but possessed of a worm's specialized mentality, and endowed with a worm's necessities. It is inconceivable to us how the bat can sense and determine the exact position of a mosquito in the dark with an exactness greater than eyesight. It is inconceivable to us how the bloodhound can follow the faint footprints of a man or animal with unerring accuracy from an odor so inconceivably faint as to be beyond our imagining.

It is no more inconceivable, no stranger, that the nervous structure of a lower order of animal may be so sensitized to special environing influences as particularly to be constituted to perceive or feel out the mental processes of an enemy that is hunting him for prey, and to take the hint from his enemy that would cause him, as I have pointed out, to imitate the very thing his enemy has told him would protect him if he looked enough like that thing.

Life is the coördination of the motional relations constituting the character of a body with the motional relations constituting the character of other bodies.

The motional relations constituting the character of every body must of necessity be sensitively responsive to the motional relations constituting the character of all other bodies in measure proportionately correspondent with the chief necessities of the body. In other words, life is the functional response of an organized body to the forces of surrounding matter, termed the environment.

A living being is a body of matter organized to utilize the forces and properties of surrounding matter for its functions of life and reproduction.

The condition of everything in existence acts to modify the condition of everything else in existence, according to their inter-related necessities, with an intensity proportionate to their necessities, their nearness to one another, and their developed mutual receptivity.

Mimicry in animals has always been an unsolved mystery—a mystery popularly deemed unsolvable except on the hypothesis of an all-wise, all-powerful, omnipresent, personal God, who, by direct interposition, specializes animals to adapt them to the necessities of their environment. But I think that the problem admits of scientific solution. I think that I can explain the mystery.

Knowing that everything in existence is sensitized to everything else in existence, we know, therefore, that there can be no state of being which does not tend to impress its condition upon or betray its condition to other existences whose necessities develop in them faculties of awareness—senses to meet their requirements—besides those senses that we count on the fingers of one hand.

When I was a boy, I often used to catch bees by the two wings while they were sucking honey from a flower, and thereby hold them in such a position as to prevent them from stinging. Sometimes I would miss my bee, catching him by one wing, and then I would get a sting. One day I caught what I supposed to be a bee, and missed getting him by both wings. I was surprised that he did not sting me. On examination, I discovered that he was not a real bee at all, but a species of fly. I have since learned that that fly was a representative of the class of animals that finds protection in mimicking other animals. That species of fly, like a bee, lives mainly upon the honey of flowers. Fly-catching birds, through long ages, fed upon the ancestors of that fly. Each time one was caught, he was asked the question, with a look of the bird, whether or not he were a bee or a fly, and he took the hint, and by slow degrees became to look more and more like a real bee, so that he found greater and greater protection in his looks, and the largest numbers of those flies survived that looked the most like bees. So complete is the resemblance now as to afford very efficient protection against being devoured, for the bird does not want to take any chances on his turning out to be a bee and able to deliver a sting in the throat.

Our higher and more complicated development of mind and body does not better qualify us, but, on the contrary, largely disqualifies us to perceive with certain senses as keenly as do lower animals, with their more highly specialized senses.

The olfactory mechanism of the dog is far superior to ours. The engineering skill of the spider transcended that of human beings until recent times. The bat possesses an absolutely additional sense, of which we have no counterpart: in the depths of a cave half a mile by a winding way from its entrance, a place wholly devoid of light, bats will flit about the head of an intruder, but will always avoid coming into collision, as though they were swallows in open daylight.

Many an insect whose necessities have caused him to specialize in the development of a telepathic perceptive sense is far superior to us in the exercise of such faculty.

Anciently, we may very possibly have had a more highly developed telepathic sense, but as we have evolved means for communication, especially through speech, we have supplanted one kind of sense-perception by another kind more suited to our purpose, with the result that certain telepathically-perceptive faculties have now become largely atrophied.

Similarly, we have lost our former aptitude for grasping and handling objects with our feet, because our hands have served the purpose as grasping organs, and our feet have become specialized better to adapt them to their present use. We were once able, doubtless, to flap our ears to dislodge flies, but we no longer possess that faculty. We once possessed strong and heavy jaws armed with long fangs, for tearing meat and for peeling nuts and fruits, and our hands were armed with claws, but we have no longer any need for claws and fangs, because we are better armed with ingenious intelligence.

After making many experiments in an attempt to ascertain whether ants are susceptible to sounds within the range of the human ear, and finding that they are apparently deaf to such sounds, Sir John Lubbock came to the following conclusion:

> It is, however, far from improbable that ants may produce sounds entirely beyond our range of hearing. Indeed, it is not impossible that insects may possess senses, or sensations, of which we can no more form an idea than we should have been able to conceive red or green if the human race had been blind.

It is an irrational conclusion that an insect, simply because he is an insect and smaller than we, does not know his business as well as we know ours. As a matter of fact, many an insect knows his business as well for his needs as we know ours, and the action of certain insects can not be other than rational, involving planning and calculation.

The word *instinct*, as a distinction between the intelligence of man and of lower animals, should be abandoned for all time. Instinct in the lower animals is but inherited experimental knowledge. Much of what we know is likewise inherited experimental knowledge. The child who learns with great facility something for which his immediate progenitors possessed especial aptitude, acquires but part of the knowledge by his own efforts; the rest comes to him as instinctive knowledge inherited from those progenitors.

I once saw, on the porch of my residence on Lake Hopatcong, a mud-hornet deliberately fall into and entangle herself in a spider-web. The spider, perching upon an outer corner of the web, instantly sprang at the hornet, then stopped, and decided that it did not want to tackle that hornet, and returned to its perch. After waiting awhile for the spider to come to the attack, the hornet freed herself very easily from the web; and I watched her fly several times in circles, and then deliberately alight in another nearby web, and entangle herself in it. Instantly, the alert spider, evidently either more hungry or less cautious than the other, sprang upon the hornet, when, with an alacrity that would shame the lightning, and with a precision developed beyond the contingency of error, that hornet seized the spider, jabbed her sting into it and paralyzed it. Then she did it up nicely and carried it away.

I learned afterward, in the study of insects, that this is the regular habit of the mud-hornet—that she catches spiders in this manner, paralyzing them with her sting. She places them one after another, in a mud-pocket that she has constructed for the purpose, until she has enough canned spiders to feed her young when they hatch out in the spring. The spiders do not die, but remain alive in their prison until attacked by the larvae of the hornet and eaten at the proper time. Rather hard on the spiders—but the habits of the spiders themselves are not such as to elicit much sympathy.

Another day, I was watching a spider's web on the porch of my country house, hoping again to see a mud-hornet play the same trick on a spider. After long waiting, I was rewarded for my vigil. A mud-hornet jumped into the web of a spider, pretending to entangle herself in it. The spider made a dash for the hornet, but drew back a little distance, and regarded her cautiously, finally cancluding that it was too risky. After waiting awhile for the spider to make the attack, the hornet pretended to struggle again and to entangle herself inextricably in the web. I thought, "Madame Hornet, you have overdone it this time." The spider thought the same thing, and attacked the hornet. No magician ever unbound himself from a knotted rope with the cleverness with which that hornet released herself from the web, and it was all done with the quickness of a cat striking with its paw.

A condition of mind is a physical condition. It is a physiological condition. It is as much a physical, chemical, electrical phenomenon as is the production of a spark from the discharge of a storage battery exploding a gas mixture in an internal combustion engine. It is as much the result of arrangement of atoms and molecules as is the formation of a frost-crystal. It is as much an electro-mechanical phenomenon as is the establishment of an electric current in an induction coil.

It is a no stranger phenomenon that a certain electrical condition produced in the mind of a bird on seeing a worm should influence the receptive mechanism in the terminal ganglion of a worm, a butterfly, or a beetle than is the phenomenon that a current of electricity will by induction set up a current in a distant coil absolutely without physical contact. It is no stranger a thing that there should be ultra-Hertzian waves than it is that there are Hertzian waves.

Since we are able, through wireless telegraphy, by means of Hertzian waves, actually to operate a mechanism and record thought at a distance of several thousand miles, we certainly have a right to suspect that the worm or the butterfly may have a nerve-apparatus capable of catching vibrations set going by the thinking mechanism of a bird of prey, and of interpreting their meaning and of profiting by the interpretation.

In this age of experimental investigation, when, as far as possible, we put everything to a practical test before shaping our conclusions in regard to it, it may appear at first sight that the conclusion that a worm can read the mind of a bird is rather fanciful and chimerical. As we go back, however, over the steps of our reasoning which led us up to this conclusion, it does not look so chimerical or so fanciful.

We know positively that no mental phemomenon can take place without leaving an impress of some character upon surrounding media, because no condition of any structure can exist without that condition making its impress upon surrounding media, tending to alter the conditions existing in those surrounding media. Consequently, we know with absolute certainty that a thought can not exist in the mind of a bird without that thought exerting an influence upon other life in the

*From the *North American Review*.

neighborhood of the bird, and that one thought exerts a different influence upon the mental mechanism of a worm than will another thought. These things we know; the only thing we do not know, and can not very well prove by practical experiment, is that the worm is able to utilize that influence in the way I have suggested.

If a man could live a million years and experiment with birds and worms and butterflies, as Darwin experimented with doves and other animals to prove his theory of natural selection, we might prove our theory of the cause of mimicry to the extent of showing that certain animals do assume the guise of other animals, or mimic them, for purposes of self-protection against their enemies, but we should not know then any better than we now know that the change was produced in the manner I have suggested. Although this conclusion must, of necessity, be entirely theoretical, it is certainly a rational theory, and one which should be accepted as most likely to account for the strange phenomena of mimicry in lower animals.

There are many well-authenticated instances which strongly tend to warrant the conclusion that certain lower animals possess peculiar psychic powers not usually understood—powers by which they are able to interpret the bent of mind of human creatures in whose presence they happen to be.

Many a time, when the owner of a dog has determined to kill the dog in order to get rid of him for some reason or other, the dog immediately seems to discover the fact that there is some evil purpose brewing for him, and he will often slink away and hide without any apparent cause. Of course, the voice of the master in calling the dog may change, and the dog will discover a sinister meaning in the difference of the voice, or the master's countenance may change, that is to say the master may manifest in his face his intentions regarding the dog. But there have been many instances when there has been no opportunity for the dog to tell by the change in his master's demeanor or change of voice, and he has seemed to gather a warning directly from the operations of his master's mind.

I admit that there may never have been a single instance in authentic proof of this conclusion, but there have been instances enough to lead to a strong suspicion that the mind of a dog may be in such receptive telepathic attitude with respect to the mind of his master as to interpret the bent of his master's thoughts concerning the dog's welfare; and the dog's welfare is the principal thing that can concern the dog.

In arriving at such conclusions as these, it is necessary for us to be strongly on our guard and to maintain alert vigilance against being humbugged by the deceit of others or by our own sentiments and imagination.

When we desire a thing, it is much easier to believe that thing than when we do not desire it. In spiritualistic investigations, this human peculiarity has given the wildest fancies the guise of scientific evidence.

In the interpretation of any natural phenomenon, we must not take our lack of ability to understand its having been produced in any other way as proof that it must have been produced in a certain hypothetical way. This we must admit applies to my suggestion of the explanation of mimicry in animals. We have not sufficient evidence to know whether this hypothesis is true or not, only enough to know that it is both possible and rational and based upon the broad foundation in fact that nothing can exist except under the domination of the universal influence of all things else, of which influence it is a sensitized reciprocal part; and that there must be a mutual influence exerted between the mental mechanism of lower animals existing in the neighborhood of one another. We further know that those animals have developed on the lines of their chief necessities; that, consequently, they are capable of utilizing any influence to meet their necessities.

Therefore, may we not conclude that it is reasonable to believe that hunted animals must have used any influence exerted upon them by hunting animals in the development of mimicry for defense, if that mimicry has been a necessity?

The Use of Titanium in Steel Castings*

By W. A. Janssen

NOTWITHSTANDING all that has been said concerning the harmful effects of phosphorus and sulphur in steel, the occluded oxides and gases, such as iron oxide, Fe_2O_3, and an undefinable oxide, probably FeO, free oxygen, nitrogen and occluded slags are the real causes of many of the troubles of the steel-maker. It is with the occurrence of these elements and their elimination that he is especially concerned. It has been definitely demonstrated that the presence of oxygen, and possibly nitrogen, in steel reduces its static strength, dynamic properties and abrasive values and increases its tendency to corrode. Today the presence of oxygen and oxides in steel is considered more harmful than even relatively large amounts of phosphorus and sulphur. In a measure the same is true of nitrogen, although the investigations in this direction have not been sufficiently complete and the results are variable and uncertain.

With the advent of ferroalloys of silicon and manganese containing high percentages of the active elements came the hope of an assured uniform quality of steel. As deoxidizers, modern ferroalloys are efficient in a measure, but have certain limitations. It is the function of these deoxidizers to combine with the occluded oxides; the resulting products rise and become a part of the slag. Unfortunately the temperature of the metal and its resultant fluidity do not always permit these to complete their cycle and rise to the slag; they are entrapped as inclusions and the occluded gases are not entirely deoxidized. The known presence of oxides in excess of the amount which can be deoxidized by the usual additions of ferrosilicon and ferromanganese is not permitted because of specification tolerances for manganese and silicon content.

Silicon, comparatively speaking, is not a strong deoxidizer, and when it is added to steel a portion of it remains in the steel either as an alloyed constituent, or the products of its oxidation may remain as inclusions. The usual analyses for silicon do not disclose whether or not the silicon is present in the steel as an alloyed silicide, as silica or as the silicate. Even if the silicon manifest itself as a silicide, showing a high silicide percentage, a wild heat is apt to result, requiring the use of a further deoxidizer (aluminum) when pouring the molds. In conjunction with manganese, double silicates of iron and manganese frequently are formed. Such a constituent may contribute to excessive segregation, although singularly, a dirty steel often discloses very little segregation.

Titanium, until a comparatively few years ago looked upon as one of the rare metals, undoubtedly is one of the most powerful deoxidizers and denitrogenizers known. At the present time it may be obtained as one of the ferroalloys. Its chief value lies in its positive action in the removal of the occluded oxides, nitrogen and entrapped slags, due to the fusibility of titanic oxide as formed and its greater stability as compared with iron oxide. Its function is further augmented by the increased fluidity due to the increased temperature because of the exothermic reaction, thereby permitting freer movements of the oxidized products to slag.

The present-day method of using ferrotitanium is to augment the incompleted cycle with ferrotitanium after the other deoxidizers have been added. These may be added in the ladle, or in the furnace before tapping. After the titanium has been added, it is *imperative* and *essential* that the ladle be held from 5 to 10 minutes before pouring in order to allow time for the completion of the reactions. No fear need be had of the chilling of the metal inasmuch as the temperature is raised appreciably, due to the exothermic reaction. It is essential that the titanium be not added until after the additions of ferrosilicon and ferromanganese have been made. On account of the greater affinity it had been tried in England, and even before it was well under way. England has the honor of starting both the acid and the basic Bessemer, but it was left to the United States to show the world what an acid vessel could do, while Westphalia was to develop the possibilities of the basic converter. It is rather hard to explain just how this all came about, but the facts are clear.

Looking for a moment at the acid Bessemer in our own country, we know that from the beginning almost every works made from two to five times as much steel as the best plants across the water. For many years Europeans refused to believe that we told the truth about our output, and even a personal visit by some of their metallurgists would not convince them that operations could be carried on every day continuously under whip and spur. In the late nineties, however, the facts were too well known to be disputed, and an English plant that desired to increase its output installed American equipment in its Bessemer department. Any of our foremen, with a nucleus of American workmen, could have doubled the tonnage in two months, but the British steel workers refused to wake up, and it was necessary to build a second plant and run both of them at what we would call half speed.

The Germans have done for the basic Bessemer just what we did for the acid converter; but they have not reached and never can reach the rate of production that is so common in this country, because in a basic plant there are so many little things to watch all the time and so many extra operations. The adding of the lime, the decantation of the slag and the delay that seems to be necessary after the addition of the recarburizer, all make for slower work, while, as already stated, the converter lining has a shorter life. Under these conditions it is a notable achievement when a plant of three 20-ton converters turns out 35,000 tons of steel in a month.

There are also technical problems at every step, for a variation in the proportion of silicon in the pig iron will mean a change in the weight of lime added; again, any variation in the speed of the blowing engine makes a difference in the length of the overblow, while there is no sharp warning corresponding to the drop of the carbon flame, to mark the end of the operation. Worst of all is the danger of excessive rephosphorization when the recarburizer is added; for rephosphorization always takes place to some extent, say as much as 0.02 per cent, and it may be three times as much if conditions are not just right. Finally, the composition of the slag must be kept constant so that it can be sold as a fertilizer. All these technical and practical problems were worked out in Germany long ago, and all successful basic Bessemer practice is a copy of what has been done on the banks of the Rhine.

Here in the United States we are apt to forget how important a part the basic converter plays in the steel industry. In 1913 it made nearly one-quarter of all the steel of the world, and almost as much as the acid Bessemer and the acid open-hearth put together. During the ten years from 1903 to 1913 the world's output from the acid converter increased only 8 per cent, but the production from the basic vessel nearly doubled. A great part of this basic Bessemer steel comes from Belgium and from that portion of France which is in the war zone; so all figures for the last two years are worthless. But there is every reason to suppose that production will be resumed and will increase soon after the declaration of peace.

*From a paper presented at a Convention of the American Foundrymen. From a report in *The Iron Age*.

Solvents of Coal

THE author records extraction experiments with pyridine and quinoline on a series of coals, and results of examination of the residue and extract. The coal was powdered to pass a sieve of 900 meshes to the sq.cm., dried *in vacuo*, and extracted with boiling pyridine in a Soxhlet apparatus. Generally a large apparatus of copper (silver-plated to prevent attack by pyridine) was used. Glass rods were placed in the basket carrying the charge prior to filling; on removing them the charge was loosened. Common salt or potassium sulphate was mixed with the coal to prevent caking during extraction. Though unaffected by the pyridine, they could be extracted with water from the residue at the end. The extract was concentrated by distillation *in vacuo*, and added to an excess of dilute hydrochloric acid which produced a brown precipitate. This was washed with water and dried *in vacuo*. The residue insoluble in pyridine was treated with water and hydrochloric acid and dried.

Generally speaking, the pyridine extract is large when the volatile matter is high, but there is no close parallelism. The volatile matter in the residue, compared with the original coal, is also reduced, excepting in those cases where the extraction is slight. The small increase in such cases is probably accidental and due to retention of pyridine. The residue has either reduced coking properties or else none at all, but extract and residue mixed will still coke. The pyridine extracts are brown powders, insoluble in water, acids, and alkali, but partially soluble in organic solvents giving fluorescent solutions. The extract is soluble in fuming nitric acid, and if the extract be first suspended in glacial acetic acid, oxidation is minimized. The product, on precipitation with water, resembles the original substance but is more combustible. The pyridine extract and also coals low in volatile matter, even anthracite, behave similarly. The coal from Lens (mine 8) was also extracted with quinoline. The extract at 120° C. was more than 4 times greater than with pyridine at about the same temperature. At the boiling point of quinoline (238° C.), 5.56 per cent of the coal was extracted. At this temperature decomposition seemed to occur in addition to simple solution, for the residue had lost all coking properties and these could not be restored by mixing the extract with the residue. Ultimate analyses of the pyridine extract and the original coal were almost identical. The hydrogen and nitrogen contents of the extract were higher in the former case, due perhaps to retention of solvent. The methods of metallographic analysis were applied to some of the samples. A polished specimen of coal from Lens (mine 8) which gives a small pyridine extract, showed no change of structure after etching with pyridine. A sample from mine 3 showed dark areas which became more marked after etching. A sample of coal from Frankenholtz (seam X), which showed a perfectly polished surface before attack, disclosed granular bands after pyridine extraction.
—Note in *Journal of the Society of Industry* in a paper by A. WAHL in *Bull. Soc. Chem.*

Metallurgical Processes in the Foundry*

Use of Ferromanganese—Ferrosilicon—Chills and Iron Molds

By Alexander E. Outerbridge, Jr.[1]

BEFORE proceeding to the subject proper of my paper this evening, I wish to make a little confession. After almost a lifetime spent in the study and daily practical application of investigations in metallurgy on a somewhat large scale, more particularly in cast iron in industrial works, I feel sometimes, that I know less about the subject today than I thought I did nearly forty years ago. This is not surprising, however, for I believe it is the universal experience of investigators that the more one delves into Nature's mysteries the more one finds to discover, thus relatively, the more one studies the less one feels that he knows.

The history of the study of astronomy affords, for example, a striking illustration of this. Long before Galileo pointed the first telescope at the heavens, in 1609, the old monks had, with infinite pains, mapped out the firmament and recorded the positions of all the stars then visible to the eye of man and they naturally thought that their charts were complete. But the moment the new and more powerful eye of the telescope was pointed at the blue vault hundreds of thousands of starry worlds came into view whose existence had never before been suspected, revealing glowing stars so far distant that the light from them must have started on its long journey centuries before the Christian Era.

So it is with more mundane affairs, such as the study of the composition and physical properties of metals and alloys. Chemistry has told us pretty well the elements and proportions of the components of complex alloys, like cast iron, but that is, after all, a very dim light to guide us and it only reveals a few of the most conspicuous properties, leaving many others yet to be discovered by those who come after us.

In comparatively recent years the microscope has supplemented chemical analysis and has given us much valuable information regarding the effect of heat treatment upon the structure of metals especially iron and steel. The spectroscope also has shown us the signatures of all metals, written in fine lines of colored lights that cannot be imitated or disguised.

The specific gravity of alloys and the remarkable changes in density that may be effected by heat, as in the case of the extraordinary growth of gray cast iron when subjected to repeated heating and cooling,[2] or the equally remarkable change in density caused by sudden cooling from the liquid to the solid state, as shown in the tread or periphery of a chilled cast iron car wheel are subjects of great interest for study. These are but a few of the aids to the study of metals now available and a vast unexplored field yet remains for the future investigator.

*A paper read before the Philadelphia Foundrymen's Association.

[1]Metallurgist, Wm. Sellers & Co., Philadelphia.

[2]Two bars of gray cast iron were shown that were cast in one mold from one ladle and were both originally the same size, viz., 14-13/16x1x1 inches. The specific gravity of the metal was 7.21, making 450 pounds to the cubic foot. One bar had been permanently expanded or caused to "grow" while in the solid form to 16½x1⅛x1⅛ inches, making an increase of 40-97/100 per cent. The specific gravity of the expanded bar is 5.54 making 350 pounds to the cubic foot. These identical bars were first shown before this association in 1903. Other bars of gray cast iron have been caused to increase in cubical dimensions over 50 per cent by repeated heating and cooling, still retaining their metallic qualities.

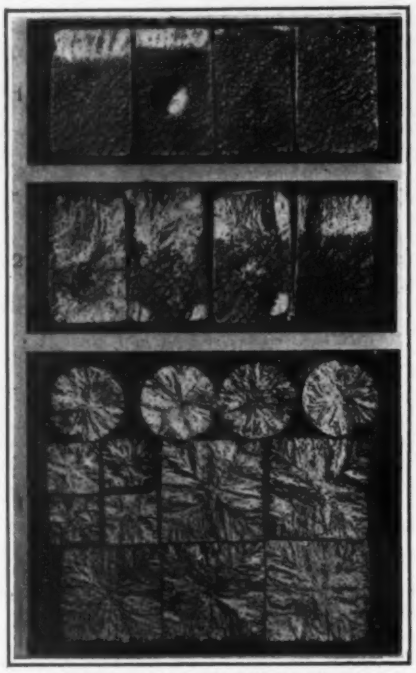
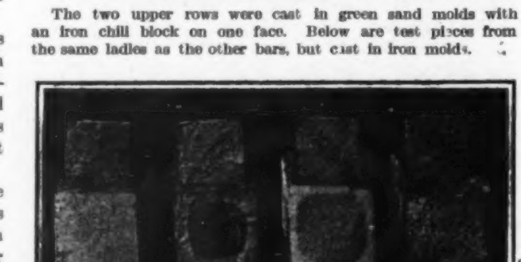


Fig. 1.—Test pieces from eight different melts of iron

The two upper rows were cast in green sand molds with an iron chill block on one face. Below are test pieces from the same ladles as the other bars, but cast in iron molds.



1 2 3 4

Results of adding ferrosilicon

1 Untreated, upper cast in green sand, lower in iron cap; 2 Same iron with 0.3% ferrosilicon added: 3 with 0.5% ferrosilicon; 4 with 0.8% ferrosilicon.

The difference between wrought iron, steel and cast iron, is commonly attributed mainly to the proportion of carbon, silicon, sulfur, phosphorus, manganese, together with a few other elements, composing these alloys, and the analytical chemist has no difficulty in determining the relative proportions of these elements with sufficient accuracy, but beyond this lies a region of which he knows nothing. He does tell us, it is true, that carbon exists in iron in two different forms, "combined" and "graphitic," but how many other forms of carbon may there be united with these other elements?

Take pure carbon alone, we already know of no less than three totally different forms, viz. (1) A soft, intensely black substance called "lamp black" which the chemist tells us is 100 per cent carbon; (2) An intensely hard transparent crystal, the diamond, also 100 per cent carbon; (3) Pure plumbago or graphite, an unctuous material, quite different from the other forms, also 100 per cent carbon. There is absolutely no chemical difference between them.

In like manner it is known that pure silicon exists in three distinct forms or amorphous conditions. So it is with some and probably all of the other elements combined with iron.

The chemist or physicist is not yet born who can, by any known methods of investigation, state with positiveness—or even guess at—the various possible forms or combinations of the elements composing these alloys and it is not, therefore, surprising, that oftentimes the analyses of two specimens of iron, steel or other metals, will be nearly alike, when the physical properties are very different.

These are a few of the problems I have been studying for nearly forty years, but you are, of course, interested mainly in the practical side of such questions, hence we will now descend from the realm of theory to that of practice.

We will first have thrown upon the screen a photographic reproduction of a number of cast iron test pieces made to illustrate an address given by myself before the Franklin Institute in 1888, on "The Relation Between the Physical Properties and Chemical Constituents of Pig Iron." (Fig. 1). Here are shown test pieces cast from eight different melts of iron. The two upper rows were poured in green sand molds having an iron chill block on one face.

The test bar at the right-hand upper corner shows merely a "skin chill" on the part cast against the iron block, the next bar shows a very little more chill, the other bars show increasing proportion of white iron on the upper faces and two show white iron on the lower faces also. Beneath the eight test pieces cast in sand, you see nine square and four round test pieces, *all perfectly white iron*, showing very clearly that the crystals are always formed at right angles to the surface of the mold. These test pieces were cast in *iron molds from the same ladles of iron as the other bars* and you will notice that all are equally white, notwithstanding the fact that the bars cast in sand with an iron chill block on one face vary enormously in depth of chill.

These iron molds may be called "chill magnifiers." I have used similar chill cups daily for detecting chilling properties in soft foundry irons for over thirty years, and they have proved to be of the greatest value in detecting chill that would not otherwise be suspected in such iron mixtures.

The main difference in composition of the eight melts of iron shown in this plate was found to be in the relative proportion of silicon.

The test piece showing a mere trace of chill has over 1½ per cent silicon, the one showing the most chill has about 6/10 of 1 per cent silicon, the other test pieces contain silicon ranging between these extremes.

The manufacture of chilled cast iron car wheels, a purely American invention, to which the development of railways in this country may be said to owe its rapid progress in the early days and to which it is still largely indebted for freight traffic, was rendered possible by the practical application of the chilling property found in our cold blast charcoal irons, and its continued success today is dependent largely upon the skill of the chemist to simulate those natural qualities by other means, long after cold blast charcoal iron has gone out of existence.

My first investigation of this peculiar property of

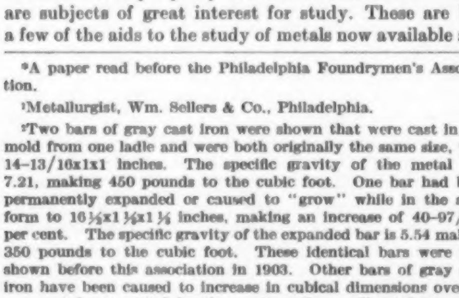


Fig. 3. Iron mold for saddle

For 36-inch planing machine and casting made in this mold. The mold was made in 1903. This casting was a difficult one to make in a sand mold so as to get clean surfaces on both sides

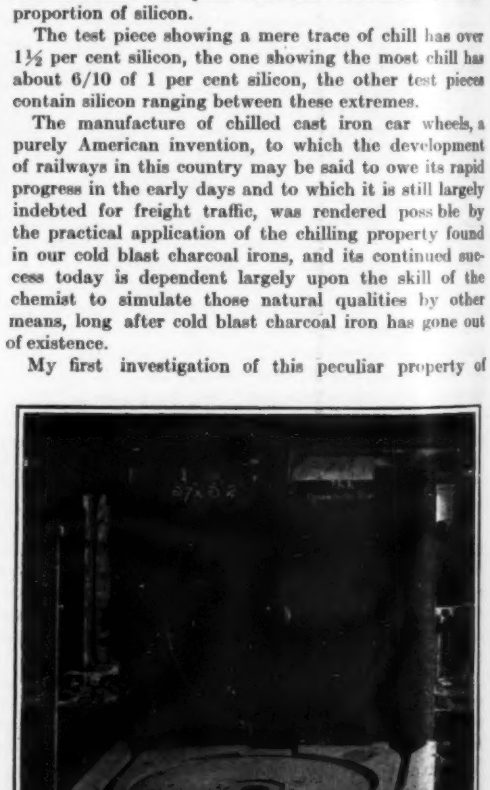


Fig. 4. Iron mold for friction disks for dynamometer for Pennsylvania railroad

Forty-two-inch diameter, cast 1½-inch thick, finished all over ⅝-inch thick. The pull on the draw bar of the locomotive on this machine was originally 80,000 pounds, afterward increased to 100,000 pounds. It was essential that these cast iron disks should be fine grained and free from the slightest defects. After failing to procure satisfactory castings in sand molds they were cast in this iron mold and were perfect disks. Casting poured from bottom.

Fig. 5. Iron mold with collapsible central portion

Gear ring for 42-inch car wheel lathe. Main driving gear, teeth cut in ring which is cast solid. Cast open. "Semi-Steel" is poured in these molds. Over 100 rings have been cast in this mold. The blank ring is 5-foot diameter, 8-inch deep and 6-inch thick.

cold blast charcoal iron (and of some grades of anthracite and coke iron) dates to the back year 1876, when a fine exhibit of chilled cast iron car wheels at the Centennial Exhibition first attracted my attention. One of the wheels was broken to show the fracture; the plate and brackets were dark gray, while the tread of the wheel was white as silver.

On inquiry I learned that the white iron tread was formed by sudden cooling of the metal cast against a heavy iron "chill ring" while the plate and hub of the wheel, cast in a green sand mold, were quite soft, the tread was hard as steel.

At that time I was making some specific gravity determinations of various metals in the assay laboratory of the mint, where I was an assistant, and samples of the white iron forming the tread of a wheel and of the dark gray iron forming the body were furnished to me for examination. I was astonished to find that the white metal weighed about sixty pounds to the cubic foot more than the gray iron portion of the same casting.

This served to explain the abnormal cooling strains in a chilled casting of this character, necessitating prolonged annealing, or very slow cooling, to enable the molecules to adjust themselves gradually and thus relieve the strains.

These investigations led to others, and for a period of eight years I was daily engaged in the study of the metallurgy of chilled cast iron car wheels and their manufacture. It was customary to cast, each day, in the car wheel works where I was engaged as metallurgist, a large variety of chilled car wheels ranging from full size, double-plate passenger and freight wheels, 33 inches diameter, street car wheels, (usually of spoke pattern) down to small and light mine car wheels.

It was not possible to pour all of these from one mixture of iron for the metal suitable for the heavy wheels was too high chilling for the smaller wheels. This necessitated melting soft iron every day in a small cupola to mix in varying proportions with the regular iron in separate ladles.

To overcome this expense, if possible, by altering the character of the iron in individual ladles, after delivery from the cupola, a great many experiments were made (1880–4) with aluminum (then costing $8 per pound) spiegeleisen and 80 per cent ferromanganese, granulated or powdered, added in small quantity to the metal in the ladles.

The effects produced by aluminum and spiegeleisen were in the right direction, though they were not sufficiently marked to be valuable, but it was then found that when powdered 80 per cent ferromanganese was added in the proportion of one pound to 600 pounds of iron in a ladle, a most remarkable change was immediately produced in the character of the molten iron in the ladle. The first publication of these experiments was given in the address already alluded to, printed in the *Journal* of the Franklin Institute, March, 1888, and was as follows:

"A remarkable effect is produced upon the character of hard iron (*i.e.*, high chilling iron) by adding to the molten metal, a moment before pouring it into a mold, a very small quantity of powdered 80 per cent ferromanganese, say one pound in 600 pounds of iron. The result of several hundred carefully conducted experiments which I have made enables me to say that the transverse strength of the metal is increased from 30 to 40 per cent, the shrinkage is decreased from 20 to 30 per cent and the depth of the chill is decreased about 25 per cent, while one half of the combined carbon is changed into free carbon."

Prior to this time manganese had always been regarded as a hardening element in cast iron, and the surprising claim that when added in a ladle of high chilling car wheel iron it had an opposite effect at first met with incredulity, but in course of time its use became almost universal and I believe it still is used very largely indeed in the manufacture of chilled cast iron car wheels. No application for patent rights was made for this process.

Ferromanganese, when added in powdered form to a ladle of molten car wheel iron, acts as a deoxidizing and desulfurizing flux by reason of the strong affinity of manganese for oxygen and sulfur, but it is only efficacious when added to iron that is comparatively low in silicon (less than 1 per cent) and high in combined carbon.

It is absolutely useless, if not positively harmful, when added to foundry mixtures carrying over 1 per cent silicon and but little combined carbon, yet I know that 80 per cent ferromanganese has been sold to foundrymen for treatemnt of their foundry iron mixtures during the time when the alloy could be bought for a few cents a pound. Its present price is prohibitive, except for special purposes.

The use of ferromanganese as a flux in ladles in car wheel manufacture is of enormous value to that industry,

Fig. 7. Iron mold for "over-pig"

The molds were first made in 1880 and this mold has been in constant daily use for many years. On top of mold are two iron molds for "lifting nuts" for cross-head of large boring mills, from 10 to 16 feet. These casting must be made of very strong, fine-grained iron. Also on top are two test bars originally of the same size. Viz: 14-13/16-inch x 1-inch x 1-inch cast in the same mold from one ladle of iron. One bar has been caused to "grow" or increase in size to 16½-inch x 1¼-inch x1¼-inch, equivalent to 40-98/100 per cent enlargement.

Fig. 8. Iron mold for casting long core rods

In use since about 1890. So designed that it never warps. Many thousands have been cast in this mold.

Fig. 9. Iron mold "waffle iron"

Design mounted on trunnions for casting "gaggers." In use since about 1890. So designed that it never warps. Many thousand gaggers have been cast in this mold.

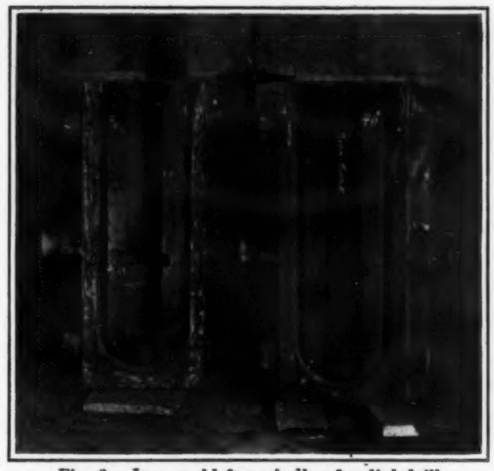


Fig. 6. Iron mold for spindle of radial drill

Mold, 40-inch long, 16-inch wide, 12½-inch thick. Spindle, 4½-inch diameter, 2-inch core, 1¼-inch thick metal. Poured from the bottom.

its abuse through ignorance or failure to understand its proper functions must have led to disappointment when added as a flux in ladles of foundry iron by those who do not understand or appreciate the radical difference between car wheel iron and foundry iron, whether soft or hard.

While the manufacture of chilled cast iron car wheels requires very accurate control of the chilling property of the metal, it is of far more uniform character than the iron needed for the great variety of castings, ranging from a few pounds up to perhaps 30 tons or over that must be melted often in one day, in the large machine tool works with which I am connected as metallurgist. It is customary, therefore, to group all the small work needing soft ductile iron so that it will be cast near the beginning of the run of iron from the cupola, this is followed by a medium grade of iron for miscellaneous castings, capable of being machined readily, but not too soft to give good wearing service; this again is followed by strong iron mixtures intended for large and heavy work requiring metal of high tensile and transverse strength.

The importance of having some simple, cheap and reliable method of changing the physical properties of molten iron after it has been tapped from the cupola has been regarded as a desideratum ever since the value of the method of modifying the chilling property of car wheel iron in individual ladles, by treatment with powdered ferromanganese, became well known. This led indeed, as I have stated, to improper use of ferromanganese in ladles of foundry iron through ignorance of its proper functions.

As long ago as 1880 I made efforts in this direction with alloys of iron and silicon, but at that time the richest alloy obtainable contained less than 20 per cent silicon.

In later years 50 per cent ferrosilicon was produced in electric furnaces for use in steelmaking, and these tests were then renewed with the results that have been already published, showing that the addition of a small quantity of the powdered 50 per cent ferrosilicon alloy in a ladle of molten foundry iron produces a remarkable effect upon it, by *softening* and at the same time *strengthening* the iron. This is quite an opposite effect to that produced by increasing silicon in the cupola, which always tends to weaken iron.

The following table shows the results of a few tests of this kind made in 1905 with ordinary foundry irons:

TABLE I—RESULTS OF TESTS OF FERRO-SILICON IN LADLES

Date	Remarks per Cent Si	Breaking Strain (Lbs.)	Deflection (In.)	Resilience	Shrinkage (In.)	Per Cent Gain: Breaking Strain	Per Cent Gain: Deflection	Per Cent Gain: Resilience
Oct. 10, '05	¼ lb Hand Ladle Gain	2265 2155 110	120 .112 .008	45.30 40.09 5.21	.192 191	5.1%	7 14%	13%
Oct. 11, '05	¼ lb Hand Ladle Gain	2215 2165 50	.137 .124 .013	50.51 44.60 5.91	.182 .185	2.3%	10.4%	13.2%
Oct. 14, '05	¼ lb Hand Ladle Gain	2600 2055 545	.152 .116 .036	65.79 39.18 26.61	.196 .188	26.5%	31%	67%
Oct. 15, '05	¼ lb Hand Ladle Gain	2330 1893 437	.143 .110 .033	55.45 34.60 20.85	.183 .183	23.8%	30%	60.2%
Oct. 16, '05	¼ lb Hand Ladle Gain	2210 2120 90	.124 .112 .012	45.54 39.44 6.10	.183	4.2%	10.7%	15.4%
Nov. 13, '05	1 lb 200 lb Ladle Gain	2370 2050 320	.139 .118 .021	54.76 40.19 14.57	.186 .184	15.6%	17.8%	36.2%
Nov. 21, '05	1 lb 200 lb Ladle Gain	2178 2000 178	.128 .119 .009	46.34 39.60 6.74	.188 .182	8.9%	7.5%	17%

The effect of adding four ounces of powdered 50 per cent ferrosilicon in a hand ladle holding about twenty-five pounds of molten iron, and of adding one pound to 200 pounds of iron in a ladle, upon the strength and deflection, as compared with the same iron untreated, is clearly shown in this table. The success of these initial tests led to more extended investigations. We will now throw upon the screen a photograph showing the remarkable effect in reducing chill in foundry iron by adding thereto powdered 50 per cent ferrosilicon in different proportions in the ladle, ranging from 3 to 8/10 of 1 per cent of silicon.[1] (Fig. 2.)

No. 1 shows the fracture of two test bars cast from a ladle of untreated iron. The upper piece was cast in green sand, 1 inch x 1 inch x 15 inch, and was an ordinary transverse test bar. The lower piece was cast in a heavy iron cup in order to very rapidly cool the metal. The upper test piece is perfectly gray and easy to drill. The lower one is perfectly white and can not be drilled with an ordinary steel drill.

No. 2 shows the fracture of two similar test bars poured from the same iron after addition of 0.3 per cent of silicon in the form of high grade ferrosilicon (50 per cent silicon) added in the ladel. Note the marked reduction in depth of chill in the sample cast in the iron cup.

No. 3 shows the fracture of two similar test pieces from the same iron after adding 0.5 per cent silicon in the ladle. The "chill" is still further reduced.

No. 4 shows the fracture of two similar bars from the same iron after adding 0.8 per cent silicon in the same manner. The chill in the sample cast in the iron cup is almost entirely absent.

The samples cast in sand are all perfectly gray, but there is an appreciable difference in the fracture, the treated metal showing softer iron than the untreated sample.

The metal selected for this particular test was comparatively low in silicon, consequently having comparatively high chilling property, but this fact would never be suspected from an examination of the fracture of the test sample cast in sand. Very few practical founders are aware of the large variation in chilling property of their iron, sometimes of presumably similar grades, melted on different days, or even of the wide variations which may occur in different portions of one heat.

Here we have, I think, a practical solution of one great difficulty that has heretofore militated against the successful use of permanent iron molds for making castings that must be machined.

A mere "skin chill" not thicker than a piece of tissue paper makes the surface of a casting too hard to machine, but by this simple process the foundry foreman can eliminate the last trace of chill from his metal before pouring it into an iron mold.

For years past more or less encouraging efforts have been made and described in technical journals of methods for making castings in permanent iron molds. The most ambitious one, perhaps, was the pipe casting process of Mr. Custer, at Tacony, a few years ago, for which he obtained several patents. Mr. Custer ingeniously overcame the trouble from skin chill on his castings by turning them out of the molds before the molecules had taken a final set and the intense heat in the body of the casting helped to anneal out any skin chill that might have formed. Mr. Custer failed, unfortunately, to take into due consideration the fact that gray cast iron, of which his molds were made, "grows" or increases in cubical dimensions when repeatedly heated above a certain point and then cooled. This, I am informed, caused such a rapid deterioration of his casting machines that his process proved a failure and has been discontinued.

It will, no doubt, be a great surprise to you to learn that ever since the year 1888 machinable castings have been made in the foundry of Wm. Sellers & Co., Inc., in permanent iron molds, and still are being so made. I am, through the courtesy of the President of that company, now permitted to show you photographs made for this address of original iron molds, still in use together with some machinable casting that were made in them years ago. Also large and small molds of more recent manufacture. In fact, more than a hundred permanent iron molds, ranging in weight from a few pounds up to 7,000 pounds each, are to be found in use in this foundry.

The main object of casting in iron molds is to secure machinable castings of exceedingly fine grain, free from dirt, sponginess or other defects, while at the same time having marked increase in strength.

The question of relative cost of castings made in permanent iron molds (eliminating the molder entirely) as compared with similar castings made in green sand, or in baked sand molds, depends upon the number of machinable castings required to be made from one pattern as well as upon other conditions.

We will now show a picture of an all-iron mold made in 1903, for a saddle for 36-inch planing machine, also one of the gray iron machinable castings absolutely free from any skin chill, made in this mold. (Fig. 4).

This was a difficult casting to obtain free from surface defects on one or the other face, or from draw holes, when made in sand, but when cast in this iron mold, they were perfect, and all the saddles subsequently made, until a change was made in design, were cast in this iron mold.

This mold was made 15 years after my first iron mold for machinable castings was made and used in the foundry of Wm. Sellers & Co., and 23 years after the first iron mold was made by me for casting "over-iron" in another foundry, and thereby hangs an amusing tale.

In the year 1880, I suggested to the foreman of the car wheel foundry of A. Whitney & Sons, Philadelphia, with which I had become connected as metallurgist, the idea of casting all the "over iron" in iron molds instead of in open sand molds, thus giving more room on the foundry floor and avoiding expense of making up a sand pig bed daily.

The foreman exclaimed in alarm, "Why it will be all *white* iron and when we remelt it with the wheel mixture the next day the wheels will all be too hard. We do use a little white iron sometimes to bring up the chill when the mixture is running down in chill, but this plan would spoil all our calculations."

Nothing but an actual demonstration would convince the man of his error. Iron pig molds were then ordered to be made, and a ton of regular wheel iron cast therein. This iron was, of course, white. On the next day the ton of white iron was remelted in a small cupola without admixture and cast into test bars, chill test pieces, and two test wheels. On the following day, when all of these were broken, instead of being white iron, as the foreman had predicted, there was no appreciable difference found between the castings made from iron that had been chilled white in the iron molds and similar castings made from the same iron cast in sand molds. The result was that no sand pig bed was ever again made in that foundry as long as the works continued in existence.

On coming to the establishment of Wm. Sellers & Co., Inc., in March, 1888, I found the same conditions, and immediately substituted iron pig molds such as are shown in Fig. 7, which have been used daily ever since that time.

These iron molds, for over iron, anticipated the machine cast pig iron process now so generally used by many years, and naturally led to the next step, namely, making permanent iron molds for castings, as soon as it was found practicable to prevent iron poured in them from being too hard to readily machine.

We will in conclusion call attention to photographs made *in situ* for this address of a number of iron molds which have been made and used in the foundry for making machinable castings.

Anticipating the question: "How do you prevent these molds from warping or growing so as to become useless?" I will say there are two distinct methods: The simplest method, and the one we adopt, is to make but one casting from one melt in one iron mold. In this way the mold never gets red hot so that it does not grow. The second method is to use low silicon iron for the molds, as metal of that character grows far less when repeatedly heated and cooled above a cherry red than does soft foundry iron. Finally, I may say that castings made of white iron do not expand permanently when heated and cool sufficiently to overcome the original shrinkage of the metal, so that, when practicable, permanent iron molds may be made of white iron.

[1] "Iron absorbs silicon greedily, uniting with it in all proportions at least up to 30 per cent, and apparently the more readily the higher the temperature, absorbing it even at a red heat when imbedded in sand and charcoal. It diminishes the power of iron to combine with carbon. . . thus favoring the formation of graphite during slow cooling. It increases the fusibility and fluidity of iron; it lessens the formation of blow holes; by reducing iron oxide it apparently removes one cause of red shortness: it hinders at high temperatures the oxidation of iron and probably of the elements combined with it."—The Metallurgy of Steel, by Henry M. Howe.

The Menace of the Rodent

Some few years ago when plague was prevalent in California, attention was centered on certain rodents as being the probable factors in the transmission of the disease. It had already been demonstrated by painstaking and exhaustive investigations in India, the traditional home of this terrible malady, that the rat was the main agent in the conveyance of infection to human beings, but during the outbreak of plague in California, it was discovered by the energetic surgeons of the Public Health Service that ground squirrels were also involved in the dissemination of the disease. The late Professor Metchnikoff during a mission to Siberia to study the plague, undertaken under the auspices of the Pasteur Institute, came to a similar conclusion with regard to ground squirrels. Varieties of squirrels and chipmunks are believed also to play an important part in transmitting Rocky Mountain spotted fever to man by means of ticks which find a favorite lodging place on the bodies of these animals.

However, the menace of the rodent rests largely, or almost entirely with the rat, and the main disease carried by the rat is the loathsome and deadly malady known as bubonic plague. Innumerable fleas find a refuge with each rat, and when a rodent is infected with the *bacillus pestis*, a certain proportion of the verminous multitude that make their home with the sick rat in turn become infected, and thus not only perpetuate the infection but if perchance they come in contact with human beings, insure the direct transmission of the disease.

At the present time, due to conditions brought about by the war, there are numerous vessels sailing to and from New York, which provide most desirable dwelling places—from the rat's point of view. Tramp steamers of every description, some in that advanced stage of decrepitude, which especially appeals to the domestic side of the pestiferous rodent now make New York their home port. Moreover, owing to the exigencies of war, many old wooden vessels on the verge of falling to pieces from age and decay have been pressed into service, and these with their practical impossibility of disinfection provide conditions admirably suited to the rat and his filthy habits.

Needless to say the disease-carrying rodent has taken full advantage of these opportunities, and it will be no exaggeration to state that many of the ships which changed circumstances are bringing to New York, and causing them to lie in the harbor for a considerable time, are swarming with infected rats.

Some of these vessels sail from ports in which plague is endemic and although our own sanitary authorities are competent and constantly on guard, yet at the same time, the situation is menacing to some extent. So far as is known, the rat fulfills no useful purpose. It may be true that he consumes a certain amount of garbage and refuse, but the interests of public health will hardly countenance this method of garbage disposal. At any rate, it is clearly obvious that the best rat is the dead rat, and this fact may be especially emphasized under existing conditions. It has been suggested that it would be well to initiate against the rat a campaign of a world wide character with the slogan "*delendus est mus!*"

Every physician should recognize the increased menace and be vigilant accordingly.—From *American Medicine*.

Platinum in Spain

Mr. F. Gillman described before the Institution of Mining and Metallurgy (England), some investigations on the occurrence of platinum in Spain.

He stated that in 1913 Don Domingo de Orueta, during a field study of the peridotites in the Ronda highlands of the province of Malaga, was impressed by the apparent analogy between those areas and those of the platiniferous district of the Urals from which at least 90 per cent of the world's supply of platinum is derived. After a laborious petrological examination of both Spanish and Russian rocks he found his surmise confirmed, and proceeded to test the alluvial deposits and beds of the numerous rivers in the Ronda highlands, working in the mass of partly serpentinized peridotites, which extends from near the Mediterranean at Estepona to the north as far as Tolox, a distance of 25 miles, with a width of eight to ten miles.

On analyzing his sand samples, each weighing from 30 to 40 kg., he found most of them to be platiniferous. The metal generally appeared in the form of minute rounded or flat grains, though sometimes as small, more or less water-worn nuggets, with a maximum weight of about 2 grammes. As in the Ural district, the platinum is concentrated in a stratum of sand one to two metres thick, resting on bedrock and covered by a practically barren overburden varying in thickness from eight to twelve meters. Apart from samples of four borings which proved exceptionally rich, nearly one-third of the borings yielded platinum at the rate of two to three grammes per cubic meter; more than a third contained 25 to 40cgrm., while the remainder had only a few microscopical grains. At the present price of the metal, alluvial ground containing only 20 to 25cgrm. of platinum per cubic meter is profitably worked in the Ural district. The Malaga crude metal contains from 78 to 82 per cent of platinum, the residue being palladium, rhodium, ruthenium and osmiridium.

In order to determine whether the deposits can be worked industrially, Señor Orueta has obtained Government aid, and provided with mechanical drills and other equipment, has again taken the field to face two or three years more of research.

The Radiation of the Stars*

By A. S. Eddington

Since the publication of Homer Lane's paper "On the Theoretical Temperature of the Sun" in 1870, many writers have discussed the internal state of a star, considered as a globe of gas in equilibrium under its own gravitation. Recent observational work gives encouragement to these investigations, for it is now known that numerous stars are in a truly gaseous condition with mean densities similar to that of our atmosphere. To such stars the results for a perfect gas may fairly be applied, whereas stars, such as the sun, with densities greater than water must necessarily deviate widely from the theoretical conditions. The stars which are in a perfectly gaseous state correspond to the "giants" on H. N. Russell's theory,[1] or to the stars of rising temperature on Lockyer's principle of classification; the denser "dwarfs" are outside the scope of this discussion. The two series coalesce for spectral type B, which marks the highest temperature attained.

The internal temperatures which have been calculated are so far beyond practical experience that we may well hesitate to apply the familiar laws of physics to such conditions. But in so far as the investigations can be based on the second law of thermodynamics, the conservation of momentum, or laws which are directly deduced from these, there can be little doubt of the validity of the treatment. We cannot altogether avoid assumptions of a speculative or approximate character, and no doubt some of the results described in this article are open to serious criticism on that account; but to a considerable extent the discussion can be made to rest on laws which are held to be of universal application. Moreover, natural phenomena usually became simpler at high temperatures; gases become more "perfect"; the absorption of X-rays follows simpler laws than the absorption of light; the heat-energy comes to be located in greater proportion in the ether, so that the precise nature of the material atoms is less important.

Most investigators have assumed that the stars are in convective equilibrium.[2] In that case, when the mass and mean density are given, and also the molecular weight and ratio of specific heats (ϒ) of the material, we can find at once the temperature at any internal point. Let us take a star of mass 1½ times that of the sun and of mean density 0.002 gm./cm.^{3}; for illustration, the average molecular weight will be taken as 54 (*e. g.*, iron vapor dissociated into atoms at the high temperature). For ϒ we shall take $\frac{5}{3}$, but any possible change in ϒ makes comparatively little difference in the results, so far as we require them. For this star the calculated temperature at the center is 150,000,000°; half-way from the center to the boundary it is 42,000,000°. But the temperature of which we have some observational knowledge is not given immediately by these calculations; according to observation, the "effective temperature" of a star of this density would probably be about 6,500°. This term does not refer to the temperature at any particular point, but measures the total outflow of heat per unit surface. Now, the outflow of heat evidently depends on two conditions—the temperature gradient (more strictly the gradient of T^4), and the transparency of the material; therefore, the temperature-distribution being calculated as already explained, we can deduce the transparency necessary to give the observed effective temperature of 6,500°. The result is startling. We find the material must be so absorbent that a thickness of one-hundredth of a millimeter (at atmospheric density) would be almost perfectly opaque. There is little doubt that such opacity is impossible. Conversely, if we adopt any reasonable absorption coefficient, the effective temperature would have to be above 100,000°, which is decisively contradicted by observation.

A way out of this discrepancy is found if we take into account the effect of the pressure of radiation. Fortunately, this effect can be calculated rigorously without introducing any additional assumption or hypothesis. Suppose that a beam of radiation carrying energy E falls on a sheet of material which absorbs kE and transmits $(1-k)$E. It is known from the theory of electromagnetic waves that radiant energy E carries a forward-momentum E/c, where c is the velocity of light; similarly, the emergent beam carries momentum $(1-k)$E/c. The difference kE/c cannot be lost, and must evidently remain in the absorbing material. The material thus gains momentum, or, in other words, experiences a pressure. The amount of the pressure kE/c involves the coefficient of absorption k, of which we have no immediate observational knowledge; but it is the *same* coefficient which has already entered into the calculations of the opacity of the material, so that the introduction of radiation-pressure into the theory brings in no additional unknowns or arbitrary quantities.

The radiation-pressure is thus proportional to k, and to the approximately known outflow of energy. The preposterous value of k already found would, if adopted, lead to a pressure far exceeding gravity, so that the star would be blown to pieces. But the radiation-pressure modifies the internal distribution of pressure and temperature; it supports some of the weight of the outer layers of the star, and consequently a lower temperature will suffice to maintain the given density. The smaller temperature-gradient causes less tendency to outflow of heat, and there is accordingly no need for so high an opacity to oppose it. By calculation we find that for a star of mass 1.5 times the sun, and molecular weight 54, radiation-pressure will counter-balance 19/20ths of gravity; somewhat unexpectedly, this fraction depends neither on the density of the star (so long as it is a perfect gas) nor on the effective temperature, but it alters a little with the mass of the star. The pressures and temperatures are then reduced throughout in the ratio 1/20; for the star already considered, the corrected value of the central temperature is 7,000,000°. Assuming an effective temperature of 6,500° we can now calculate the new value of k; it amounts to 30 C.G.S. units, *i. e.*, 1/30 gm. per sq. cm. section will reduce the radiation passing through it in the ratio 1/e. It is of considerable interest to note that this is of the same order of magnitude as the absorption of X-rays by solid material; for at the high temperatures here concerned the radiation would be of very short wave-length and of the nature of soft X-rays.

The approximate balance between radiation-pressure and gravity leads to an important relation between stellar temperatures and densities. It is easy to put this relation in a more rigorous form; but it will suffice here to express the condition as radiation-pressure = gravity. If T is the effective temperature of the star, and g the value of gravity at the surface, the outflow of radiation (per unit area) varies as T^4, and the condition is

$$kT^4 \propto g.$$

We shall assume that k is the same for all stars. Now g depends on the mass and mean density in the ratio $M\,\rho^{\frac{2}{3}}$. Hence

$$T \propto M^{\frac{1}{12}}\rho^{\frac{1}{6}}.$$

The range of mass in different stars is trifling compared with the great range of density. Thus the leading result is that *the effective temperature of a giant star is proportional to the sixth-root of the density.* To test this, we take the densities given by Russell[3] for the different types, and, assuming that stars of the solar type (G) have the sun's effective temperature (6,000°), we calculate by the sixth-root law the temperatures of the other types.

Type	Density (⊙ = 1)	Effective temperature
A	$\frac{1}{10}$	10,800°
G	$\frac{1}{350}$	6,000°
K	$\frac{1}{2800}$	4,250°
M	$\frac{1}{25000}$	2,950°

The calculated numbers in the last column agree almost exactly with the temperatures usually assigned to these types, and it is clear that if Russell's densities are correct the sixth-root law must be close to the truth.

If a is the radius of a star the total radiation will be proportional to a^2T^4, which varies as ga^2, *i. e.*, as M. The total radiation thus depends only on the mass, and not on the density or stage of evolution. The absolute luminosity is a fairly good measure of the total radiation for the range of temperature here considered, though, of course, the visibility of the radiation changes a little with the temperature. We shall thus have the total radiation constant as we pass through the series of spectral types, and the luminosity roughly constant (with deviations amounting to about 1½ magnitudes). This is just the feature which Russell has pointed out in the luminosities of the giant stars; they are practically the same whatever the type of spectrum.[4]

It may be remarked that this theory avoids a difficulty noticed by J. Perry[5], that when ϒ is less than $\frac{4}{3}$, the heat within the contracting star is greater than the energy set free by contraction, leaving less than nothing for radiation into space; the difficulty is even more serious than Perry considered, for he did not make any allowance for the enormous store of ethereal energy necessary for equilibrium with matter at high temperatures. But we have seen that by taking account of radiation-pressure the interior temperature is much reduced; less internal heat is therefore needed; and there is, in fact, an ample balance of energy left for dissipation even when ϒ is considerably below $\frac{4}{3}$.

With the molecular weight smaller than 54 the importance of radiation-pressure is reduced; for example, with molecular weight 18 radiation-pressure is 6/7 of gravity, instead of 19/20. But it still plays a predominant part until we come down to molecular weight 2. Reasons have been urged in favor of a low average molecular weight—perhaps as low as 2. It is probable that the atoms are highly ionised by the radiation of short wave-length within the star; and if most of the electrons outside the nucleus are split off from each atom we shall actually have an average weight for the ultimate independent particles nearly equal to 2, whatever the material (excluding hydrogen). Radiation-pressure is then less than half gravity; but the two principal laws, which seem to be verified by observation, are arrived at as before. Moreover, the order of magnitude of k is scarcely altered; it is now 5 instead of 30 C.G.S. units. Nor is the internal temperature much changed. In fact the effect of ionising the atoms is that the pressure of the superincumbent layers is supported by a mixture of cathode rays and X-rays, instead of by X-rays alone; our doubt as to the proportions in which these occur and as to which will predominate is no serious hindrance, because the main results are nearly the same in any case.

*From *Nature*.

[1] *Nature*, vol. xciii., pp. 227, 252, and 281.

[2] There are strong reasons for believing that the interior of a star must be in radiative equilibrium, not convective equilibrium. The internal distribution of temperature and density is, however, of the same character in either case; if the coefficient of absorption is independent of the temperature, then the distribution corresponding to radiative equilibrium is the same as that of material for which ϒ = $\frac{4}{3}$ in convective equilibrium. See *Monthly Notices, R.A.S.*, vol. lxxvii., p. 16.

[3] *Loc. cit.*, pp. 282–83.

[4] *Loc. cit.*, p. 252, Figs. 1, 2, and 3.

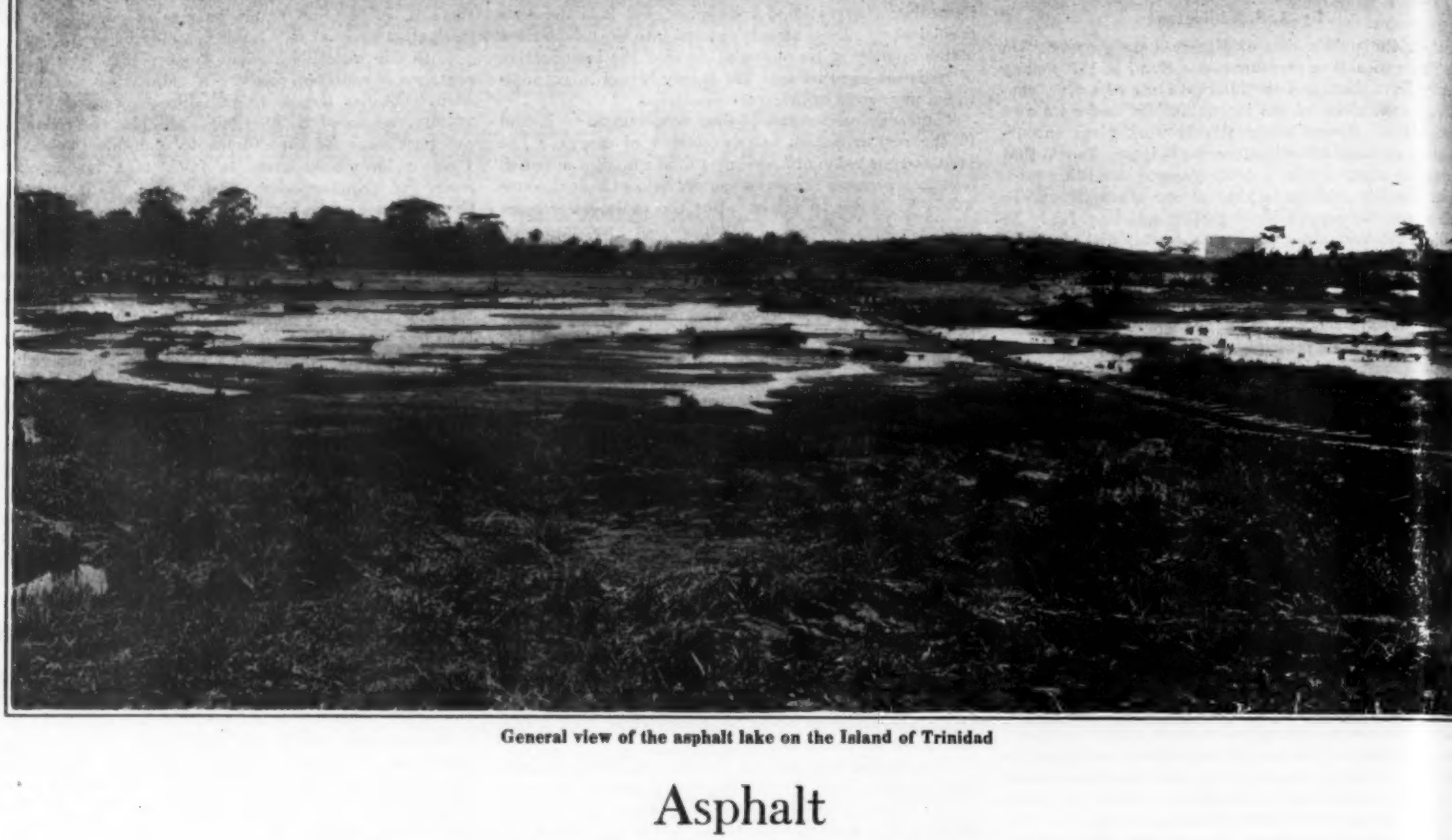
[5] *Nature*, vol. lx., p. 350.

Sources of Fat in Germany in 1916

Increased cultivation of poppy, rape, and to a less extent of flax, has yielded considerable quantities of oil. Linseed oil was used as an edible oil only in certain districts of Saxony and Silesia. Last year the bulk of the oil from this source was used for munitions, and the remainder was used in the manufacture of margarine. Sunflowers were also extensively cultivated. According to an American report from Dresden, over 300,000 kilos. of sunflower seed oil was obtained in 1915; but this estimate is decidedly too high seeing that in 1916 only 100 tons of seed was obtained from every 77 tons sown. Horse chestnuts contain in the dry condition about 6 per cent of a yellow oil which resembles rape oil and is edible. The extraction of this oil has now been undertaken by a department of the German War Office, and the residue is used as fodder. The expectations of obtaining fat from yeast have not yet been fulfilled. Experiments made at the close of last year on the removal of the germs from cereals with a view to obtaining the oil, have given much more promising results. Wheat contains 2 to 3 per cent of germs; rye 2.5 to 4 per cent; barley 2 to 3.5 per cent; oats, 3 to 4 per cent; and maize, 10 to 14 per cent. The germs contain about 12 per cent of fat and about 35 per cent of protein. The annual production of wheat in Germany is 15 million tons, and if only 1 per cent of germs was separated from 10 million tons, and 10 per cent of oil was obtained from these, the total yield of oil would be 10,000 tons. Millers are now compelled to separate the germs and to deliver them to the War Department. Rye is freed from husks in one machine and then crushed in a second machine, and the germs are separated by sifting. In the case of wheat they are sifted from the coarser and medium grains. The grain oil, when refined, is suitable for the manufacture of margarine, and for other edible purposes. In Austria the Oil and Fat Control has bought up all used coffee grounds to extract from them the 12 per cent of oil which they contain. More than 4,000 "fat-retainers" have been affixed to the sewage outlets of different towns, but so far these have not fulfilled expectation. A sample of the crude "fat" was found to contain about 50 per cent of water and dirt. When purified by boiling with dilute sulphuric acid it yielded a yellow-grey fat which would be suitable for technical purposes.—Note in *J. Soc. Chem. Ind.* on a paper by W. Fahrion in *Z. angew. Chem.*

Psychological Effects of Earthquakes

The psychological effects of earthquakes would seem to offer an interesting thesis subject for some graduate student of psychology. The mental effects upon human beings of such common natural phenomena as thunderstorms and earthquakes have not yet received the careful attention they deserve. With the occurrence of even a mild thunderstorm some persons are precipitated into a kind of hysteria, while other persons show no reaction whatever. The same may be said of earthquakes. Moreover, with certain earthquakes some people hear sounds and occasionally detect unusual odors. In recording personal impressions made by an earthquake, personal equation enters perhaps more completely than in the observation of any other natural phenomenon. The whole problem invites attention. California would seem a fitting field for a study of this nature.—From a paper by A. H. Palmer on California Earthquakes in 1916 in the *Bulletin of the Seismo. Soc. of Am.*



General view of the asphalt lake on the Island of Trinidad

Asphalt

Where It Comes From and Some of Its Characteristics

ONE of the modern wonders of the world consists of the self-replenishing lakes of asphalt. Two of these are especially noteworthy because of the attention they have attracted and because of their importance from an industrial point of view. They are both located near the mouth of Orinoco River. One is on the mainland and is known as Bermudez Pitch Lake; the other is on the nearby island of Trinidad and has Trinidad Pitch Lake for its name.

From this latter deposit 125,000 tons or more of asphalt are sent to the United States every year in addition to a smaller amount despatched to Europe. America seems to be the greatest customer. Enough has been secured from this one lake of 114 acres area to provide refined pavement material for 170,000,000 square yards of city streets in the United States. But the lake does not appear to have been exactly robbed. The surface has gone down a few feet, but only a few. This is one of the remarkable facts about Trinidad Pitch Lake. On the average, each square foot of the lake has supplied 300 square feet of pavement in the United States, to say nothing of pavement elsewhere. The lake is, in part at least, self-replenishing.

While the term *lake* is in common use when speaking of such a source of supply as that on the island of Trinidad, and is correctly used, we are not to imagine an evidently liquid mass. It is doubtful whether we should use the word *liquid* in this connection. Nevertheless, the crude asphalt of the lake is not a solid in the commonly accepted sense of that word. There is a degree of plasticity. Asphalt will flow in a deliberate, ponderous way, just as ice will flow when properly subjected to pressure, but the natural asphalt is more plastic than ice. There is reason for this comparison. After a hole of some size has been excavated in the lake, a period of twenty-four hours is sufficient usually to obliterate the depression. It is thought that this obliteration is due rather to the flowing into the hole of the asphalt surrounding the sides than to an invasion of the space from below. Probably what takes place is an inflow from every direction except from above which is caused by unbalanced pressures on all sides and the bottom. At any rate, the hole disappears and its disappearance seems due to a flowing of the material. No doubt the general subsidence of the surface which has taken place in the course of years is to be accounted for by the constant creation of such holes and their subsequent obliteration.

Further, the mass of material in the lake seems to be in some kind of movement. There are various indications of this. A line of wooden stakes or plugs was driven. The subsequent deviations from a right line noticed make clear that some movement had taken place meanwhile. A drill hole was put down some years ago to a depth of 135 feet. The hole deviated so much from verticality by the time this depth was reached, although the mode of operation was that known as wash drilling, that the work had to be stopped. In fact, there appear to be here and there separated areas each involved in a localized movement. A piece of wood will emerge from the center of such an area in a vertical position. It is then borne slowly towards the circumference, a gradual falling away from verticality occurring as this centrifugal movement goes on. Finally, the piece of wood reaches the outer edge of the area which is ordinarily, it seems, more or less depressed. Arrived here, it falls over completely and sinks into the general mass. Near the center of the lake itself, there appears to be a greater degree of activity.

Gas bubbles in the surface waters of the asphalt lake

All this is quite curious. Despite the movements going on in the mass, the surface is quite stiff. A narrow-gage railway runs out over it. The weight of the individual cars when loaded are thought to be 1000 pounds or more. However, there is but little trouble due to the sinking of the cross-ties into the asphalt. Workmen engaged in mining the asphalt from the surface and carrying it to the cars have no trouble, it would appear, due to a tendency to sink in.

We have then, here on the island of Trinidad, a lake consisting of a material which is not representatively solid nor yet representatively liquid. The areas is easily determined. The depth, no one knows. The uncompleted drill hole indicates that the depth has a maximum in excess of 135 feet. As the surface is 138 feet above the sea level, it seems probable that the bottom of the lake is as low as or lower than the ocean. There is, perhaps, no apparent significance in this. It is a point, however, that it may be as well to bear in mind.

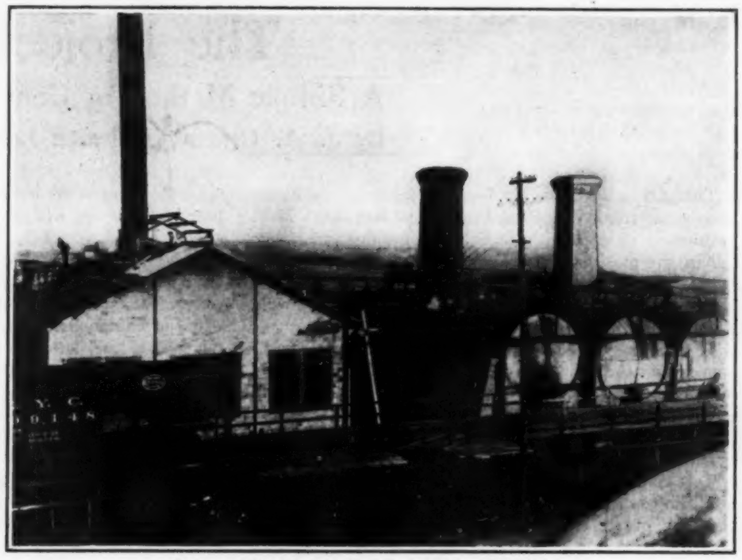
A very curious thing is the attempt, more or less successful, too, of vegetation to get a foothold on the lake's surface. Pools of water form in depressions and, along the edges of these, small trees, brushes, grass and the like are found to be growing. Small areas, each supporting more or less vegetation, are scattered about and constitute something analogous to islands.

In general, the crude asphalt is sufficiently hard to be flaked out in lumps by means of a mattock. A lump may weigh 50 or more pounds. Everywhere there are gas cavities similar to the blow holes in cast iron. The material is "an emulsion of water, gas, bitumen and mineral matter, the latter consisting largely of fine sand and a lesser amount of clay." Softer material is to be found near the center of the lake. There is a location where there is a constant inflow of soft asphalt. Gas is associated with the hard asphalt to a certain degree. With the softer material, however, there is a more vigorous movement of gas, which consists largely of carbon dioxide and methane. Further, the soft asphalt seems to generate from its own mass a gas of a different description consisting largely of hydrogen sulphide. The generation of gas from this soft material has, it seems, been established by sealing a quantity of the asphalt in a tin can and ascertaining later that the can was subsequently ruptured from within. The location where the soft asphalt makes its appearance is not fixed, but shifts from point to point. A considerable amount of water is mingled with this material. One is able to take some of it up into the hands and mold it into a ball without soiling the hands to an especial extent. The soft asphalt hardens after a time. Doubtless, it is such hardening that brings about the shifting of the point of egress at the surface of the continuing supply. It must be that we have here, in part at least, the mode and means of the constant replenishment that must be going on.

A piece of the crude asphalt will contain about 29 per cent of water and gas. The bitumen content is 39 per cent or a trifle more. There is a 27 per cent total of mineral matter. The balance of the content consists of water of hydration of clay and silicates. The preliminary refinement of the asphalt, which for some of it is accomplished on the spot, consists principally in getting rid of the 29 per cent of water and gas. The bitumen then forms 55 per cent of the total and the mineral matter about 38½.

We must not get the idea that the lake is a wonderfully hot mass of material. This is not at all the case.

A laboratory for investigating asphalts



Large stills for refining the crude material

It is understood that with a good breeze blowing, the surface has just about the temperature of the surrounding air. But, if the sun is shining vertically down upon the surface and there is no wind, then the temperature rises to a point of discomfort.

The working of the "mine" is carried on by simple hand labor. The little railroad is shifted more or less from time to time. Its loads are carried partly to the refinery which is located on the edge of the lake and lies near the sea. After the simple refining done here, the asphalt is taken down by an overhead cableway to the steamer landing. The cars or buckets on the cableway are carried down and up largely by gravity, since it is the loaded cars that are continually going downward.

But some of the railroad cars do not go to the refinery at all, but come to rest beneath the head of the conveyor cable. This is, naturally, at the top of the hill. The asphalt being contained in buckets, removable from the car framework, is readily transferred to the cableway. This is done by simply putting in proper position the hooks which are suspended from the cable and move along with it. The constantly moving cable then lifts the bucket off the wheels and conveys it to the pier. The cars that are to go to the refinery are switched off on a special track.

The refinery process consists in evaporating off more or less of the 29 per cent of water and gas contained in the crude asphalt. The material is treated in large tanks by means of coils of piping connected with a steam supply. The asphalt melts under this treatment and a good deal of the water passes off as vapor or steam. The softened asphalt is run into barrels, which are headed up and are then ready for shipment.

With reference to the origin of the asphalt lake on Trinidad, we may be pretty confident that it is due to the presence nearby of petroleum in the soil. Oil wells have been put down in the neighborhood which have opened up a big supply of asphaltic petroleum. The cavity in which the lake exists may be regarded as a natural well penetrating and opening up various strata of oil sands. But there is more than petroleum in the asphalt. There is a good deal of free water and quite an amount of fine sand. There is also clay, but in less amount. The petroleum as it rises into the cavity holding the lake is supposed to come into contact with the colloidal clay in mud below the cavity and to become emulsified with it, the resulting emulsion being crude asphalt.

Between the lake and the sea, there are other deposits of asphalt which are of minor importance. The asphalt "has been subject to atmospheric weathering and to contact with the soil for years, as a result of which its physical properties are inferior to those possessed by the lake asphalt." This material is known as "land pitch." It is understood that its degree of weathering and physical inferiority is roughly proportional to its distance from the lake.

The Trinidad petroleum seems to be quite desirable as a binding agent for use in road surfacing. It has a rather low specific gravity. There is a high percentage of what are known as light distillates. When these are removed and also the small percentage of intermediate distillates, we have a residue of a truly asphaltic character. This residual oil is suited for making hot applications in road surfacing. The natural oil, with the intermediate and light distillates still a part of its content, may be used without heating. The thin liquid penetrates everywhere. Evaporation, particularly when aided by the sun, carries off the distillates and leaves behind the heavy asphaltic content in the pores and crevices where it is desired as filler and binder.

The Bermudez Pitch Lake differs quite markedly from that on Trinidad. In the first place, it is not located in a somewhat elevated district but in a savannah bordering a swamp. The foot hills begins just back of the lake. The area is much greater than that of Trinidad Lake, being 900 acres as compared with 114. On the other hand, the asphalt goes down only a few feet. The maximum depth is said to be 9 feet. The surface is quite irregular, depressions or holes being of frequent occurrence. It is also hard. At the same time, the soft material at the center, which we regard as the material of replenishment, is thinner than the analogous material in Trinidad Pitch Lake. Various forms of vegetation are scattered about. At their roots are to be found ridges of pitch mixed with soil. These ridges are understood to result from fires in the vegetation when the latter is in a dry state. If the hard crust is removed, the asphalt below is somewhat similar to the Trinidad variety. The crust is thin in some places, thick in others the minimum and maximum being a few inches and two feet.

The water in the crude material occurs differently from that in the Trinidad variety. That is to say, there is no emulsified water and mineral matter in the Bermudez asphalt. The percentage of water is not at all uniform, but varies from 11 to 46 per cent. It is all adventitious surface water. The material carefully selected for use industrially is constant in character, however. The bitumen is softer, purer and contains no colloidal clay. However, the ultimate chemical compositions of the two bitumens are nearly the same. Both contain a rather heavy percentage of sulphur, a constituent which seems to be regarded with some favor. Apparently, the sulphur promotes hardening upon exposure to the atmosphere. In the language of Dr. D. Holde, an authority on oils and fats, the distinction between native asphalt and bitumen may be stated thus:

"A bitumen would be called native asphalt when it contains considerable amounts (2.10 per cent, usually over 4 per cent) of sulphur not removable by steam, when the amount of the latter in the chloroform extract which is insoluble in benzol is at least 7.5 per cent and the asphalt separated according to the method of Marcusson and Erckmann, contains 1.4-3.1 per cent of oil with at the most 0.6 per cent paraffine.

"A bitumen would be called a petroleum residual pitch if it contains at most 1.7 per cent of sulphur, even in the chloroform extract prepared as previously described, and further 26-59 per cent of oil in which the paraffine amounted to 3.3-16.6 per cent.'.

"It appears—that a native solid asphalt is characterized by the fact that it contains sulphur, and the same thing is true, though in a lesser degree, of the softer bitumens from which it is derived."

The "mining" and transfer to shore of the crude material is accomplished in much the same way as at Trinidad—that is to say, by hand implements and by means of a little railway on the lake and by a cableway leading down to the pier. The shipments are considerable.

Petroleum in Assam

An account of the petroleum industry in Assam was given by Mr. H. S. Maclean Jack before the Institution of Petroleum Technologists.

He said that the history of the development of the industry down to the present time is practically the history of the Assam Oil Company, which is the only concern in the country producing, refining, and marketing oil. Of the two areas worked, one, known as the Makum area, lies on a slightly elevated plateau between the northernmost of the Naga hills and the River Dehing; and the other, called the Digboi field, is nine or ten miles to the north, where a small range of hills crosses the otherwise absolutely flat valley of the Brahmaputra. For several reasons the collection of geological information in these districts is very difficult, and the first wells were inevitably drilled blindly. Surface shows, which were the chief guide, take rather a peculiar form. All through the jungle of the neighborhood are open spaces, known locally as pungs, to which the wild animals resort, and each pung is in some way connected with surface shows of oil, which is always traceable close at hand; there are usually exposures of rock, and, besides actual oil seepages, there are often fairly strong blowers of gas.

The original drillers employed were Canadians, who used the Canadian pole system. The bands of spongy clay and the steep dip of the strata gave trouble, and the wells took an extremely long time to drill and were very expensive. Improvements were consequently sought for, and among other things ropes were substituted for the Canadian poles.

The crude petroleum in Assam is found in intermittent deposits situated in loose sands. Considerable gas pressures are present, and on first striking the oil the well usually flows with a good deal of force, and in most cases will continue to flow at intervals for many months. The pure crude oil has a specific gravity of 0.856, and appears of a very dark brown, almost black color, by reflected light, without the fluorescent greenish tint common to many American oils. By transmitted light, at 84 deg. F., the color is warm sienna and the oil is quite transparent. At 82 deg. it is perfectly fluid, but at 81 deg. crystals begin to form, and at 77 deg. it is semi-solid.

After being settled in tanks the crude oil is pumped into boiler stills arranged in groups of three, crude benzine being taken off from the first, intermediate kerosene from the second, and the bulk of the ordinary kerosene from the third. From the crude benzine, by redistillation in closed steam coils, two qualities of petroleum spirit are produced, but the demand does not absorb the available supplies. Efforts to develop a demand for lighting, the spirit being vaporized and burnt with an incandescent mantle, have been fairly successful. The intermediate kerosene is a colorless distillate, and gives an oil of great power value in internal combustion engines when slightly concentrated. A small quantity of water-white kerosene is produced for Europeans, but the great bulk of the kerosene product is for native consumption and is a low-grade oil of about the color of whisky. It smokes abundantly when burnt, but, luckily, the native consumer regards this as rather an attraction than otherwise.

The residue from the boiler stills is distilled to dryness in pot stills, and the whole of the middle distillate from the latter is passed through the refrigerating plant, which includes both anhydrous ammonia and carbonic acid machines. It is found best to pass the oil through both these in succession; from the former is obtained the bulk of the wax of high melting point, and from the latter the basis of the batching oil which is oil used for softening jute fiber in the process of manufacture. The paraffin wax is one of the most important constituents of Assam petroleum, and is a hard wax of very high melting point; during the greater part of the year the difficulty is to obtain the softer waxes to blend with the harder in order to get the melting point of 135 deg. which is adopted as the standard. The pot still, when distillation is finished, contains 8 in. to 12 in. of coke, which is used as fuel under the stills.

The Projection of Light*

A Simple Method of Considering the Important Factors

By J. A. Orange, Research Laboratory, General Electric Company

The art of lighting is divided into two sections, known as ordinary lighting and optical lighting. Most problems connected with the lighting of rooms and streets fall in the one section while all such things as searchlights, magic lanterns and moving picture machines belong to the other. Projection is a convenient term which is used to denote the latter division.

The distinguishing feature of ordinary lighting problems is that the lamps used can fairly be treated as point-sources (except in the matter of glare). Projection questions on the other hand will not admit of this kind of treatment, at least if quantitative results are required.

This distinction, coupled with the unequal commercial importance of the two branches of lighting, accounts for the lack of an elementary appreciation of projection among the many who are familiar with ordinary lighting technique.

The accounts of the searchlight, etc., generally given in text-books on optics assume as a basis the use of a point-source of light. Now it may be asserted at once that the only sources of light which can be treated as point-sources in all practical circumstances are the stars. To assume offhand that any terrestrial source is a point-source or that its small dimensions will justify its treatment as such will often lead to the grossest errors.

It is true that the action of any source may be deduced by considering it as a group of infinitesimal elements and accounting for each one in turn. There is, however, another method which is far less involved and which can be followed by the general reader. While a complete treatment of projection problems employs both methods, the second is so simple and direct that it always affords a valuable check—as far as it goes—on the complex analytical method. In this respect there is an interesting analogy between questions of projection and questions of mechanism. The working of any piece of mechanism can always be examined in detail, taking account of forces and velocities in all parts; at the same time it is usually more profitable to consider in the first instance the conservation of energy in the system. It is often difficult and always laborious to find analytically the flaw in a perpetual motion invention. The existence of some flaw is self-evident, however, in the light of the conservation principle. So in projection problems, the method which is here described will often indicate at a glance that an analytical argument contains a flaw somewhere.

We must first understand what is meant technically by brightness. Avoiding the popular senses which characterize a 100 c-p. lamp as ten times as bright as a 10 c-p. and a scarlet object as brighter than a gray, we can arrive very quickly at the technical sense. The term is used of surfaces—not necessarily surfaces of imposing extent, for the finest wire is considered at times—and it is a matter of appearance as determined by the conditions of the moment. Thus white paper in a dark room has zero brightness; in average daylight the brightness would be of the order of 1/6 of a unit and in strong sunlight 15 units. Brightness may be due to self-luminosity as in the case of red-hot iron or phosphorescent paint in a dark room, or it may arise indirectly as in the case of all ordinary objects in a lighted room.

The origin of the brightness is of no significance as regards the effects which are produced. Neglecting color, which has no real bearing on the question, the degree of brightness is the only thing which counts.

Thus, the illumination values at various points due to a red-hot poker follow the same rule as do those due to a painted model of the poker illuminated in an appropriate fashion. Similarly, the illuminating effect of a d-c. carbon arc might be reproduced by a model having a crater represented in white paint. In both cases the sole difference would be one of the scale of values and would be explained entirely by the differing degrees of brightness.

Brightness does not depend at all on the distance of the observer and it is often nearly independent of the angle of viewing.[1]

An observer estimates his distance from any surface, such as a sheet of paper, by reference to the appearance of the boundary, and grain or surface markings. The same is true of his estimate of the angle of viewing, but if the paper be free from markings and the observer holds a card with a 1/4 in. hole about a foot in front of the eye, the brightness of the paper will be the only thing he can judge by and this will give him no clue as to distance or angle.

There is now the question of a suitable unit in terms of which brightness may be expressed. The most convenient system is this: consider the illuminating value of one square inch of a surface relative to points lying on a perpendicular drawn from the center of that square inch.

Confining the attention for the moment to points which are a foot or more away from the surface it is found that the illumination varies with great exactness as the inverse square of the distance. A similar illumination might be obtained by placing a small lamp of suitable candle-power in place of the one square inch of surface. The candle-power of such a lamp is a measure of the brightness of the surface and we may say that the brightness is so many candle-power per square inch. It follows that the illumination at any point in such a case will be the same whether we have one square inch of surface at three feet, or 4 square inches at 6 feet, at 100 square inches at 30 feet. The appearance of the surface as viewed from the point is exactly the same in all of the cases, or stated more strictly, the solid angle subtended is the same. By extension we can say that if the view of a surface is limited entirely by an intervening frame or window, the solid angle subtended by the window and the degree of brightness of the surface together determine the illumination received. How far the surface is behind the window or what is inclination may be is of no consequence.

Considered in this way, illumination effects are very easy to understand; it is unfortunate that many people owing to the influence of earlier training no doubt, will not leave well enough alone in these matters but must needs resort to point and ray methods which are full of pitfalls. It is worth while to labor this feature of the essential simplicity of brightness methods and a few examples will be inserted here to familiarize the reader with them.

No. 1. Suppose we have a perfectly even, clouded sky which has a brightness of 2 candle-power per square inch. A room with black walls is lighted solely by means of a hole in the roof, 10 square inches in area. The illumination at a point 6 feet directly below the hole will be $\frac{10\times 2}{6^2}$ ft. candles $=0.55$ ft. candles. If the hole were 30 square inches in area then the illumination would of course be $\frac{30\times 2}{6^2}$ ft. candles $=1.65$ ft. candles. However, at 10.4 feet below such a hole the illumination would be $\frac{30\times 2}{(10.4)^2}=0.55$ ft. candles, the solid angle being exactly the same as that subtended by a 10-square-inch hole at 6 feet.

Next, consider a point which is not directly under the opening but off in a slanting direction—say 60 deg. with the vertical. The appearance of the hole from such a point will be very different. In fact a hole of 10 square inches will have an *apparent* area of 5 square inches. The illumination at such a point, then, if the distance from the hole is 6 feet, will be $\frac{5\times 2}{6^2}=0.28$ ft. candles.

If instead of using the sky we have a large white sheet stretched, say 10 feet above the hole, the illumination conditions in the room will be unchanged, provided the brightness of the under side of the sheet is the same as that of the sky. Even further, if a piece of paper or opal glass is fitted into the hole and the brightness of this is brought to the former sky brightness by proper external lighting, again the illumination conditions are unchanged.

The idea to be grasped is this: We can determine the illumination produced at any point by a bright object if we know exactly what is the appearance of that object as viewed by an eye placed at the point in question. The "appearance" includes two factors, brightness and *apparent* size; the latter is identical with the "solid angle" subtended.

No. 2. Metallic tungsten, like all other substances, becomes self-luminous when hotter than about 500 deg. C., and gains rapidly in brightness as the temperature is raised. At a temperature of 2,000 deg. C., its brightness is some 740 candle-power per square inch. Suppose a lamp has a filament of tungsten operating at this temperature, which is to say, at this brightness. The illumination produced at any point can be deduced immediately if we know the apparent area which the filament presents towards that point and also the distance of separation. These two quantities together measure the solid angle subtended by the filament surface, but the solid angle is, after all, derivable directly from the appearance or "view" of the surface from the point in question.

The commonest type of tungsten lamp, viewed from the side, shows twelve straight pieces of wire in "full-face" view. Taking the lengths as 2 inches and the breadths as 0.002 inches, we have 12x2x0.002 square inches approximately as the total area presented to view. The candle-power of the lamp with respect to points opposite the side is therefore 12x2x0.002x740 (at 2,000 deg. C.). The farther we go away from the side of the lamp, that is by moving around so as to face the tip, the smaller the apparent area of the tungsten becomes. It is halved by the time we reach 60 deg. away from the side, roughly speaking. The candle-power in that direction is correspondingly smaller. More exact information as to the value for any point could be obtained from a photograph of the lamp taken with the camera lens at that point.

No. 3. The d-c. carbon arc is effective by virtue of a very bright circular patch on the end of the positive electrode. This patch (or crater) is of even brightness and the "distribution curves" for such a lamp are determined solely by the apparent (or foreshortened) area of the crater as seen from different points. The peculiar form of those curves is entirely due to the limited view of the crater as seen from certain standpoints the obstruction being obviously caused by one or other of the electrodes.

At this stage it is well to survey the possibilities of various light sources from the sole standpoint of surface brightness. The following table gives the rough values assigned to familiar surfaces.

TABLE OF BRIGHTNESS VALUES IN CANDLE-POWER PER SQUARE INCH

White paper in bright sunlight	15
Coal gas flame	3
Kerosene flame	0.9
Acetylene flame	30-60
Welsbach mantle (mean)	30
Carbon filament	750
Tungsten filament (ordinary vacuum practice)	1,000
Tungsten filament (ordinary gas-filled practice)	2,000-7,000
Nernst lamp glower (max.)	3,000
Lime light	2,000
Tungsten filament (special practice)	24,000
D-c. carbon arc crater, from 3 amps upward	84,000
The sun at mid-day	600,000

Two things should be noticed, the enormous range of values as between different illuminants and the approximate constancy for any particular one irrespective of size of source and consumption.

We are now in a position to appreciate a very useful generalization relating to optical devices. This may be arrived at by strict geometry but, fortunately for our present purpose, the truth of it may be grasped by reference to matters of ordinary experience. The principle referred to is this: the appearance of any mirror viewed from any point whatever, must always be what might be called a mosaic of surfaces exhibiting some or all of the brightness values of the surrounding objects.[2] It will be granted immediately that this holds in the case of plane mirrors, the "looking glasses" of every-day use. That it holds equally for all curved mirrors may be readily ascertained if a little attention be given to the polished[3] silverware of the table. Looking into the bowl of a spoon, there is usually no difficulty in identifying the brightness shown in the various parts with the brightness of the surrounding objects. Here there is a species of image effect which assists the recognition, but this is not always the case. The complicated and arbitrary curves of a tea-pot will always show perfectly definite areas endowed with the brightness of the sky; the identification of other areas with room objects is apt to be difficult owing to the diversity of the objects at hand.

Now a result of this kind, holding for the indefinite assortment of reflector curves occurring in table-ware, may well be expected to hold in the case of the few simple curves employed in optical mirrors. This is indeed so and as a result we know immediately the general possibilities of such mirrors.

*From the *General Electric Review*.

[1]A surface which exhibits this latter effect perfectly is said to obey Lambert's Law.

[2]For the moment, mirrors will be assumed to have 100 per cent reflectivity. The actual value for silver is 93 per cent and the difference may well be neglected temporarily.

[3]This applies only to articles which can be said to be "as new." The diffusion which occurs when the surfaces are scratched prevents their being considered as true mirror surfaces.

The searchlight is a familiar example. It is an instrument for producing illumination at great distances and it consists essentially of but two parts, a mirror and a light-source. The latter, as previously explained, is merely a bright *surface* of some size or other.

Suppose we consider the apparatus in use and station ourselves at the point to be illuminated. How may the illumination obtainable at that point be estimated? Why, the case is a simple parallel to those considered already and we shall observe the mirror to be sharply divided into areas *having the brightness values of the surrounding objects*. Here, however, there is the greatest disparity between those values; the carbon arc crater, assuming that to be used, will have a brightness of 84,000 candle-power per square inch while no other object adjacent is likely to have a brightness of more than 1/10 candle-power per square inch. The observer will notice only that area of the mirror which is as bright as the arc crater, all the rest will seem absolutely dark in comparison. The illumination obtained follows immediately, thus if the bright part of the mirror is apparently 20 square feet or 2,880 square inches and the distance is 5,000 feet the illumination must be $\frac{2{,}880\times 84{,}000}{5000^2}=9.6$ foot candles. We can also decide at once that if the *whole* apparent area of the mirror is 25 square feet = 3,600 square inches, the maximum illumination possible with that mirror at 5,000 feet (when any ordinary carbon arc is used) is $\frac{3{,}600\times 84{,}000}{5{,}000^2}$ = 12 foot candles. There is thus an evident limitation as regards the illumination attainable at any particular distance and there are two factors in this limitation, the apparent area of the mirror presented and the brightness of the source-surface.

Mirror size is limited by cost and unwieldiness, while source brightness is limited to the values characteristic of the types of illuminant available.

In all of this we have given no attention to the form (curve) of the mirror and the dimensions of the source-surface because these matters are not involved in the considerations given. Nevertheless, the form of the mirror is important because it determines how large an extent of source-surface will be needed in order that the mirror appearance from the point considered shall *all* be bright and hence maximum illumination be obtained. The extent of the source, beyond the degree involved in this last condition, is only significant as determining the lateral range of points enjoying such illumination, in other words the "size of the spot thrown."

It will be readily understood that the only difference between an ideal mirror surface reflecting 100 per cent and a practical one reflecting say 90 per cent is that the brightness values observed in the latter are reduced to 0.9 of their original values and consequently the illumination is 0.9 of its ideal value.

The special suitablity of the parabolic mirror and the connection between source, size and area of the illuminated "spot" are entirely matters of geometry which would be out of place here.

In passing it may be of interest to note that a polished spoon bowl (or better a round ladle bowl) will show roughly what the parabolic reflector exhibits in the highest attainable perfection, namely the conferring of brightness on a reflector by means of a relatively small surface of source. Hold the bowl of the spoon at arm's length (preferably so that a dark wall is behind the observer) and bring in front of it a scrap of white paper about ⅜ inch diameter. By moving the paper slowly toward the bowl a point will be found such that practically the whole bowl assumes the brightness of the paper. This arrangement is an exceedingly *crude* searchlight but a searchlight none the less.

The generalization which has been established for mirrors in the foregoing has its exact counterpart in connection with all transparent bodies having polished surfaces, that is excluding all diffusing (roughened, etched or frosted) surfaces. Most table glassware complies with this condition; on looking *through* any part of a water-bottle for example, we see within the outline of the bottle as a frame a mosaic of surfaces showing some or all of the brightness values of the surrounding objects—and no others.[4] The pattern may or may not be recognizable as a kind of image of the room, that depends on circumstances and is of no consequence.

A complication may arise from the fact that each colored constituent of light is treated differently, and the mosaics for the different elementary colors are out of mesh, as in poorly printed maps. But if the observer were provided with deep blue or red spectacles he would discover that the reproduction of brightness values is true for each separate spectral color, a fact which is rather obscured if no such provision is used.

Once more we can argue that results which hold for the diverse, complicated and arbitrary shapes of tableware may be expected to hold for the simple forms used as *lenses* by the optician. This is so in fact and is capable of rigid proof from first principles.

As an application of this result we may take the common post-card projector. The usual arrangement comprises a holder for the card, means of illuminating the card—that is to say of rendering the card bright—and a system of polished glass pieces known collectively as an objective.

We are not now concerned with the art involved in giving those glasses the precise curves which result in the post-card picture being reproduced faithfully on the screen or curtain.

The illumination at various points on the screen is what we are interested in and it is very easy to determine what the values will be. If we go close to the screen and look back at the objective, placing the eye well within a sky part of the picture, the whole opening of the objective will show a brightness identical with that of the white part of the card.[5] Why this should be is not perhaps self-evident but a little argument will make it clear.

That very property of the objective which leads to the faithful reproduction of the picture necessitates that a screen point, which is for example part of the pictured sky, shall derive light only from the corresponding part of the card. If the eye, looking back at the objective from that point could see any part of the objective endowed with the brightness of some separate part of the card (other than that under consideration) this condition would be violated.

Once more, then, we have a relation giving the illumination at any point on the screen. Suppose that from that point the apparent area of the objective opening is six square inches, the brightness of the opening (or the brightness of the corresponding part of the card) is 50 candle-power per square inch and the distance of the screen point from the objective is 8 feet, then the illumination is obviously $\frac{6\times 50}{8^2}$ foot-candles.

For any particular screen distance the illumination (and hence the brightness of the picture viewed) is determined by the brightness of the card and the effective area of the objective opening. Now in practice the real limit to the brightness of the card is set by the heating effect accompanying the powerful illumination which must be used. Not much improvement can be expected in the card itself; *i. e.*, to get greater brightness while using the same illumination. Water cells can be used to reduce the heating effect accompanying the illumination but they are considered a nuisance and are rarely used with this apparatus.

Turning to the objective as the other limiting factor we encounter trouble with the lens maker when we attempt to go to larger openings. Not only is there difficulty and limitation in design in increasing the opening (camera buyers appreciate an exactly parallel difficulty) but the very size of the glasses required constitutes a manufacturing difficulty.

It is not necessary to discuss the method of illuminating the card; with either the arc or a suitable tungsten lamp as ultimate source the accompanying heating effect is the limitation at present in commercial apparatus.

The magic lantern is related to the post-card projector but is an instrument with much greater possibilities. The difference is due primarily to the different kinds of pictures used; the post-card has the property of scattering light in all directions and consequently merely a fraction of this can reach the objective; expressed in other words, the brightness of any part of the card is very low relative to the intensity of illumination used. The post-card has the further disadvantage of absorbing the infra-red radiation (the so-called heat waves) almost entirely. The slide used in magic lanterns is entirely different. It may scatter a little light but there is so much which is not scattered that a vastly better economy is possible as compared with the post-card. This in itself means a reduced heating effect, other things being equal, while there is yet another reason in the fact that in the clearer parts of the slide at least most of the infra-red is transmitted without absorption. The consequent subordination of the heating effect is such that the limiting feature in magic lanterns is not ordinarily the heating of the slides, although damage from this cause may occur.

An objective is used, just as in the case of the post-card projector. To consider a pictorial slide is apt to be confusing; a plain line drawing on clear glass offers no difficulties and all other slides will produce effects on the screen which are very simply related to that produced by the drawing. (The relation is the same as that of the densities which exist in the slides.) The line drawing on the glass stops the passage of light at certain points and at all other points the slide has no effect whatever.

Now there is a combination of lenses somewhat larger than the slide, just behind it. This is followed at an appropriate distance by a very bright source surface. Following the preceding line of argument and considering the appearance of the objective as viewed from the screen we find that some or all of the objective opening is as bright as the source while the rest has the negligible brightness of the other contents of the lamp housing. Usually the conspicuously bright area can be recognized as an image of the source, although it will show the color displacement previously referred to. The view from different points on the screen will show that the bright area of the objective is not the same for all, sometimes it lies in the center of the opening and sometimes it lies more or less to one side. But the *size* of the bright part is substantially constant for all viewpoints.

It is not necessary to repeat the expression for the illumination. The limitations are clearly the brightness of the source and the area of the objective opening *serving any one point*. The question of how much objective area can be provided for this service is an optical one similar to that of providing "working aperture" in a camera or a post-card projector.

It is interesting to note, however, that in the early days kerosene flames were used (9 c-p. per sq. in.). The brightness of this source is so low that large working aperture is essential in the objective to obtain reasonable illumination. Modern illuminants like the special tungsten lamps and the carbon arc have such a vastly greater brightness that the aperture requirement is not hard to meet unless the screen distance (and with it usually the size of the screen-picture) is inordinately great.

The motion-picture machine is a comparatively complicated piece of apparatus although it is closely related, optically, to the magic lantern. The lantern slide, which is about 3 inches long, is replaced by a similar picture, on celluloid, about 1 inch in length. In present practice the picture is held in place for a small fraction of a second, an opaque shutter then intervenes and cuts out the screen illumination entirely while the picture is being replaced by another, the shutter moves away and the projection of the second picture proceeds for a fraction of a second.[6] The apparent illumination on the screen is an average of the effects of this regular succession of exposures. The best result which is practicable is an average illumination of about 60 per cent of that attainable if these alternations did not occur.

Comparing the motion-picture machine with the magic lantern, the same kind of limitations are found to govern the illumination on the screen.

Again we have the working opening of the objective and the brightness of the source. However, if the screen distance and the size of the screen picture are the same in the two cases, the objectives required will be very different. Just as in the case of cameras, where a large plate means a long camera with a big lens and a small plate a short camera with a small lens (if the conditions are at all similar) so in the present case the large lantern slide can be provided with an objective proportionately larger than can be provided for the motion-picture film—equal quality of design being assumed.

As a consequence, the objective opening used in motion-picture work would be small as compared with that found in magic lanterns, and hence the screen illumination would be small, were it not for the fact that the lens makers have pushed the objective design further for the motion-picture outfit and thus reduced the disparity. To some extent this has involed a rather inferior sharpness in the projected picture but to detect this requires a critical examination which motion-pictures do not receive, for obvious reasons.

The typical outfit of today, using the carbon arc as the source, is arranged in such a way that no part of the screen gets the benefit of the whole actual opening of the objective. It is possible by using different arrangements to secure that every part of the screen is served by the whole objective opening. The gain in working aperture is sufficient to offset the difference in brightness which exists between the arc crater and specially designed tungsten lamps. This means that in the great majority of cases it is possible to attain the present illumination values if we replace the present arc arrangement by a suitably arranged tungsten lamp and leave the objective as it is.

The injury to the sharpness of the picture which follows the increase of working aperature involved in this change is not such as would be noticed by the lay observer; even if it were so, the present objectives do not represent the limit of design and their cost is a minor item in the outlay which motion-pictures demand.

[4] Losses due to reflection and absorption are here neglected.

[5] We are neglecting certain losses which occur in all lenses; viz., by reflection and by absorption. This applies also to the magic lantern and moving picture machine but in none of these cases is the effect very large. Furthermore, the corresponding factors are easily introduced by any one who is interested in the matter.

[6] Actual practice is more complicated, in order to prevent flicker.

A Qualitative Determination of the Reflection Coefficients of Some Metals in the Schumann Region*

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The great majority of the investigations of the optical properties of metals have stopped short at λ 1850. This wave-length constitutes a natural boundary in the spectrum, as the absorption of air makes necessary, for work with light of shorter wave-length, the use of a vacuum spectroscope and Schumann plates with accompanying complications. If we proceed farther into the region of extremely short wave-lengths, we soon reach a point (λ 1250) beyond which light is transmitted by no solid substance, so far as is known. We are therefore soon forced to use in any optical system, not refracting, but reflecting members exclusively. It is then evident that a knowledge of the reflection coefficients of various substances is necessary, before apparatus is designed for work in the region of extremely short wave-lengths.

The work which has been done upon the reflection coefficients of metals for light of wave-lengths lying between, say, λ 2800 and λ 1850 may be briefly reviewed. Schumann,[1] in collaboration with L. Mach, published an account of an investigation he made on alloys in different proportions of aluminium and magnesium. The work is entirely qualititaive and extends as far as λ 1850. Two alloys, No. IV (Al 1.25, Mg 1) and No. V (Al 27, Mg 24.3), reflected the ultra-violet better than anything else he tried. Hagen and Rubens[2] have done a great deal of work upon metallic reflection, but they did not include the region shorter than λ 2500. They studied eleven metals and with one exception found that they were characterized by low reflection coefficients (less than 50 per cent) for λ 2500. At this point they found magnalium to have a reflection coefficient of approximately 67 per cent. Glatzel[3] measured the reflecting power of silver, steel, nickel, and copper as compared with Brandes-Schünemann alloy. He does not state explicitly the shortest wave-length with which he worked but it seems probable that it was about λ 2380. Beyond λ 2500 none of the metals tried was superior to the alloy. Nutting[4] investigated the reflection coefficients of ten metals by a photographic method and gives graphs showing variation of reflection coefficient for the region extending from λ 4000 to λ 1850. At λ 1850 he ascribes to aluminium and brass a reflection coefficient of 65 per cent. The other metals fall below this, with 35 per cent for speculum metal. At approximately λ 2200 the absorption of the gelatine of the photographic plate begins to play an important rôle, and it seems improbable that quantitative results of much accuracy can be obtained beyond this point without the use of Schumann plates.

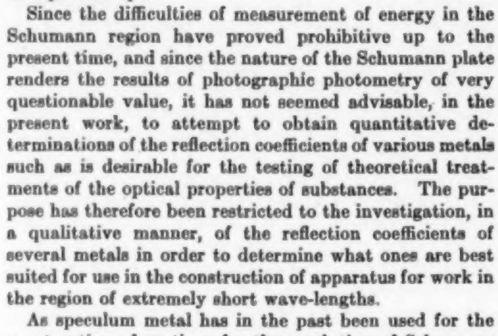
In all of the work described above the reflection coefficient has been determined by the "direct" method. There is a second method, the "indirect," in which the reflection coefficient is derived from a measurement of the constants of the elliptical polarization produced when a beam of plane-polarized light is reflected from the substance in question. A very convenient method of doing this has been devised by Voigt[5] and applied by Minor[6] Meier,[7] Fréedericksz,[8] Erochin,[9] and Foersterling and Fréedericksz.[10] These five papers provide data obtained by the same method for some twenty different metals extending in some few cases as far as λ 2260 and in nearly all cases to λ 2573. In general, the reflection coefficient for the shortest wave-length measured lies between 20 and 40 per cent. The following values are perhaps worthy of mention: silver, λ 2263, 18.4 per cent; platinum, λ 2573, 37.1 per cent; mercury, λ 2570, 57.9 per cent; Wood's metal (composition not given), λ 2573, 52.7 per cent; chromium, λ 2570, 69.8 per cent; and tin, λ 2570, 71.4 per cent.

The most recent and by far the most extensive work upon the reflection coefficients of metals which extends to λ 1850 has been carried on by Hulburt.[11] He used the direct method and measured the intensity of light by means of a photo-electric cell. Twenty-eight metallic mirrors were tried, namely, aluminium, antimony, bismuth, cadmium, carbon, carborundum, chromium, cobalt, copper, gold, lead, magnalium, magnesium, molybdenum, nickel, palladium, platinum, selenium, silicon, silver, speculum metal, steel, stellite, tantalum, tellurium, tin, tungsten and zinc. For each of the metals tried there is published a graph showing reflection coefficient as a function of wave-length between λ 1850 and λ 3800. With one exception, carborundum, the trend of the curve as we pass to the region of shorter wave-length is downward. In the case of carborundum the reflection coefficient is 15 per cent at λ 3800 and rises to 20 per cent at λ 1850. Furthermore, with one exception, silicon 60 per cent, the reflection coefficient is 30 per cent or less at λ 1850. Magnalium, for which, in view of the work of Schumann and Mach, and of Hagen and Rubens, referred to above, we should expect a large reflection coefficient, reflects less than 10 per cent of the light at λ 1850.

There are no data dealing with the optical properties of metals for wave-lengths less than λ 1850. Lyman[12] has recently extended the spectrum to λ 600, and it herefore seems desirable to continue the investigation of reflection coefficients into the region beyond λ 1850. The minimum wave-length dealt with in this paper is λ 1030, at which point we come to a second natural boundary in the spectrum. By the use of extremely pure hydrogen and an oscillatory discharge, the spectrum may be extended to λ 900, and the extension to λ 600 was achieved by substituting an atmosphere of helium for hydrogen in the spectroscope.

Since the difficulties of measurement of energy in the Schumann region have proved prohibitive up to the present time, and since the nature of the Schumann plate renders the results of photographic photometry of very questionable value, it has not seemed advisable, in the present work, to attempt to obtain quantitative determinations of the reflection coefficients of various metals such as is desirable for the testing of theoretical treatments of the optical properties of substances. The purpose has therefore been restricted to the investigation, in a qualitative manner, of the reflection coefficients of several metals in order to determine what ones are best suited for use in the construction of apparatus for work in the region of extremely short wave-lengths.

As speculum metal has in the past been used for the construction of gratings for the resolution of Schumann light, it was adopted arbitrarily as the standard reflecting metal with which the other metals were compared. We obtain then, not the reflection coefficient directly, but determine at once what metals are superior to speculum metal for the making of gratings and other optical parts.



A bit of speculum metal and the metal to be tested were so mounted that their polished faces were adjacent and in the same plane. They were then placed in such a position that the light from the quartz discharge tube was reflected (angle of incidence approximately 45°) through the slit and upon the grating. One half of the slit was illuminated by light reflected from speculum metal, the other by light reflected from the metal to be tested. For light of the wave-length to be studied, the astigmatism of the grating in the first order spectrum is so slight that we obtain simultaneously two spectra side by side, the relative strengths of which enable us to determine by inspection which is the better reflector. The method is illustrated in detail in Fig. 1, which is a horizontal section taken through the vacuum spectroscope and discharge tube. L is the discharge tube which serves as source of light, M is the composite mirror, and G is the grating. The spectrum is received on the photograpic plate P. The grating used has a radius of curvature of 102 cm, and is ruled with approximately 6000 lines to the centimeter. The ruled portion is 2.8×5.3 cm. By photographing the two spectra at the same time we eliminate any effect due to variation of intensity of the source. There remains an outstanding error due to possible unsymmetrical placing of the source in such a manner that the two mirrors are unequally illuminated. This may be largely eliminated by interchanging the two mirrors and taking a second exposure. This method, while simple, is sufficiently accurate for the purpose of the work. For if the reflecting powers of two metals are so nearly identical that the eye detects no differences in the two images on the Schumann plate, obviously one metal is, so far as the reflection coefficient is concerned, as suitable for use in the construction of apparatus as the other.

The vacuum grating spectroscope used was patterned very closely in its essentials after the one used by Lyman.[13] It differed in that it used a much larger plate (2.5×8.5 cm) which in turn made necessary the very large hole for the removal of the plateholder. This hole was closed by a conical plug 9.6 cm in diameter. The reflection of the light by the mirror necessitates the placing of the discharge tube on the side of the spectroscope instead of on the end plate. A novel feature, perhaps worthy of mention, is the mounting of a miniature incandescent lamp on an arm so that it may be swung down immediately in front of the center of the grating. If the mirrors are in correct adjustment the light from this lamp passes through the slit of the spectroscope, is reflected by the mirrors down the capillary of the discharge tube, and the image of the slit may be seen by looking through a telescope pointed along the axis of the capillary. It would have been extremely difficult to test the adjustment of the mirrors without some such arraagment.

The quartz discharge tube which served as source of light was of the external capillary type. It is shown in section at L, Fig. 1. The electrodes were of aluminium and the internal diameter of the capillary was about 0.6 cm. There is no window at the inner end separating the interior of the discharge tube from the interior of the spectroscope. The spectroscope is commonly filled with hydrogen at a pressure of approximately 1.8 mm and the spectrum obtained is that of hydrogen. The spectrum usually extends from λ 1640 to λ 1030 and is filled with lines very close together.

The directions for making plates given by Schumann[14] were followed quite closely. A device for dropping the plateholder controlled by a magnet on the outside and similar to that used by Lyman[15] was provided so that four exposures might be taken on one plate without opening the spectroscope. The lengths of the four exposures were usually 30, 60, 120 and 240 seconds.

The methods has been applied to nine substances, namely, silicon, gold, stellite, platinum, copper, nickel, aluminium, silver and steel. The majority of the metals were selected because the ease of working and permanence of surface seem to make them adaptable for the construction of mirrors, if they should prove to be good reflectors. Silicon was chosen as a direct result of the work of Hulburt, who attributed to it a reflection coefficient of 60 per cent at λ 1850. The aluminium was very difficult to polish. I was led to test it by the fact that Nutting (*loc. cit.*) gives it a reflection coefficient of 65 per cent at λ 1850. Following will be found a complete account of the preparation of the mirrors and the results obtained with them.

The speculum metal mirrors used in this work were sawed from a broken grating and polished by Alvan Clark & Sons, Corporation. They were repolished from time to time as conditions demanded.

Silicon.—The specimen of silicon was obtained from the Carborundum Works at Niagara Falls and was sawed into slabs and polished by means of rouge on a beeswax lap at the laboratory. Silicon polishes easily and the surface is permanent and does not tarnish. Silicon is much the best reflector of all the substances that I have tried. Throughout the entire range (λ 1640—λ 1030) it is a better reflector than speculum metal, although the difference is less marked for wave-lengths shorter than λ 1300. Between λ 1600 and λ 1300 I estimate that the reflection coefficient of silicon is twice as large as that of speculum metal. This estimate is only approximate and indicates that, to the eye the spectrum reflected from silicon with a two-minute exposure is fully as strong as that reflected from speculum metal with a four-minute exposure.

Gold.—The gold mirror was prepared by deposition from a cathode. Gold spatters very rapidly, and from six to ten minutes are sufficient to produce an opaque film. The spectra reflected from gold and from the speculum metal are very nearly identical in strength. Between λ 1300 and λ 1030 gold is perhaps slightly superior, but the difference is not sufficiently marked to encourage the use of gold for mirrors.

Stellite.—The stellite mirrors were cut from a block of stellite obtained from the Stellite Works, Kokomo, Ind. It is an alloy, chiefly of chromium and cobalt. The exact composition is a commercial secret. The mirrors were polished by means of diamantine on a beeswax lap. Hulburt found stellite superior to speculum metal between λ 2000 and λ 3000. His diagram indicates that its reflecting power falls off as we approach λ 1850. The spectra reflected from the speculum metal and stellite are identical in strength. In the Schumann region there is no advantage in substituting stellite for speculum metal.

Platinum.—The platinum mirrors were deposited on glass from a cathode. About thirty minutes were required to deposit an opaque film. Between λ 1600 and

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*The Astorphysical Journbal, Vol. XLV, No. 1.

[1] *Sitzungsberichte der K. Akad. der Wiss. in Wien*, 108, IIa, 154. 1899.

[2] *Zeitschrift fur Instrumentenkunde*, 22, 42, 1902.

[3] *Physikalische Zeitschrift*, 2, 176, 1900.

[4] *Physical Review*, 13, 193, 1901.

[5] *Physikalische Zeitschrift*, 2, 303. 1901.

[6] *Annalen der Physik*, 10, 581, 1903.

[7] *Ibid.*, 31, 1017, 1910.

[8] *Ibid.*, 34, 780, 1911.

[9] *Ibid.*, 39, 213, 1912.

[10] *Ibid.*, 43, 1227, 1914.

[11] *Astrophysical Journal*, 42, 205, 1915.

[12] *Ibid.*, 43, 89, 1916.

[13] *The Spectroscope of the Extreme Ultra-Violet*, Longmans, Green & Co., 1914, p. 34.

[14] *Annalen der Physik*, 5, 349, 1901.

[15] *Op. cit.*, p. 35.

λ 1300 platinum is not superior to speculum metal, in fact one or two of my negatives indicate that it is slightly inferior. But beyond λ 1300 the spectrum reflected from the platinum is distinctly the stronger. It would be very interesting to investigate the reflecting power of platinum throughout the region discovered by Lyman (λ 1250–λ 600). As noted above, silicon is a much better reflector than speculum metal in the portion of the spectrum with which this article deals, but, as compared with speculum metal, its reflecting power is growing less as we pass to the shorter wave-lengths, whereas in the case of platinum the reflecting power is increasing.

Copper.—Copper spattered readily on glass from a cathode. The mirrors tested were quite opaque. The copper mirrors which I made were markedly inferior to those of speculum metal through the entire region. Hulburt finds that at λ 1850 the copper was slightly better as a reflector than the best speculum metal mirror which he tested.

Nickel.—About three hours were required to produce a brilliant opaque film on glass by cathode deposition. Between λ 1600 and λ 1300 the reflection from nickel and from speculum metal is the same. Beyond λ 1300 the nickel is very slightly superior to the speculum metal.

Aluminium.—Two pieces of aluminium cut from commercial sheet stock were polished by Alvan Clark & Sons Corporation. It was very difficult to polish, but a surface was finally obtained which was quite good. Aluminium reflects very poorly in the Schumann region. Only a few of the stronger groups of lines show in the four-minute exposure.

Silver.—The silver mirror was cut from a coin and polished. Speculum metal is much superior to silver over the whole range upon the plate. At the end of longer wave-length the speculum metal reflects twice as well as does the silver.

Steel.—The steel mirror was hardened and polished by Alvan Clark & Sons Corporation. Steel reflects markedly better than speculum metal from y 1600 to approximately λ 1300. At λ 1200 the speculum metal reflects slightly better than the steel.

Conclusion.—The reflecting power of nine metals, silicon, stellite, copper, nickel, gold, platinum, aluminium, silver and steel, has been investigated in the portion of the spectrum lying between λ 1600 and λ 1030. The purpose has not been to determine the reflection coefficients on an absolute scale, but rather to determine directly what metals are superior to speculum metal for the construction of reflecting surfaces to be used for light in this region. The results indicate that:

1. Silicon is much the best reflector of all the metals tried, and over a portion of the region it reflects the light approximately twice as well as speculum metal.
2. The reflecting power of platinum as compared with that of speculum metal is increasing as we pass to shorter wave-lengths. It may possess advantages over other metals for work beyond λ 1030.
3. Stellite, copper, nickel, gold and steel show only minor variations of reflection when compared with speculum metal.
4. Silver and aluminium are very much poorer than speculum metal, with silver ranking above aluminium.

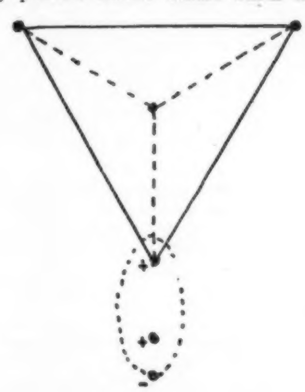
Peat as Locomotive Fuel

Reports from Sweden appear to be very favorable as regards the use of peat as a fuel for use upon locomotives, the peat being dried and powdered, and employed in the latter state, which is affirmed to be the best for firing locomotive furnaces. The government railroad administration appointed a commission within a recent date in order to make a thorough examination of powdered peat as a fuel, and after a series of tests upon the preparation and combustion of this substance, the commission reported in its favor, stating that the results were conclusive. The report established the fact that powdered peat can be utilized as a good source of fuel and concluded that it should be employed on a large scale. Accordingly the Swedish government is taking measures to erect a plant of considerable size near the peat fields of Lake Vetter for the purpose of making peat dust. As to the use of this fuel on locomotives, it is stated that all the locomotives on the 60-mile Falköping-Nassjo railroad line are now running on peat fuel with great success. The official tests showed that peat fuel as regards calorific power (by weight) is about two-thirds the value for coal. The results which are already obtained are claimed to justify the erection of large plants to utilize this national resource. In order to keep this production clear of all complicated questions regarding competition from coal, the government decided to have the new plants operated by the state, at least for the present, and is now engaged upon the plans for utilizing several large peat beds. For instance the Hästhagen fields would afford some 20,000 tons of powdered peat per annum, this to be used for the railroads, and on this basis the beds would last 20 years.

A Kinetic Hypothesis to Explain the Function of Electrons in the Chemical Combination of Atoms*

By William A. Noyes, Department of Chemistry, University of Illinois

Beginning with Davy[1] and Berzelius, during the first part of the nineteenth century, chemists generally accepted the theory that chemical combination is due to electrical forces, but when Dumas discovered the chloroacetic acids in which chlorine atoms, supposedly negative, replace positive hydrogen atoms it was believed that the theory had been shown to be false and it was practically abandoned. Following this, for fifty years or more, a theory of valence which took no account of electrical forces was developed and while occasional reference was made to positive and negative atoms and groups, no definite meaning in an electrical sense was attached to these expressions. Helmholtz in his Faraday lecture in 1881[2] drew the attention of chemists once more to the very close connection between chemical forces and electrical phenomena and spoke for the first time of "atoms of electricity." He also pointed out that the "sulfur of sulfuric acid must be charged with positive equivalents of electricity." In 1887 Arrhenius proposed his theory of electroyltic dissociation and with the help of Ostwald and van't Hoff the belief in a separation of molecules into electrically charged parts in solutions was rapidly accepted. J. J. Thomson[3] gave precision to the atomic character of electricity in 1897 when he demonstrated the material character of cathode rays and the very minute mass of the corpuscles carrying negative charges. Van't Hoff[4] seems to have suggested for the first time that electrically charged atoms may play a part in reactions not usually considered as ionic. The same idea was proposed by the author[5] and by Steiglitz,[6] a little later. J. J. Thomson[7] seems to have been the first to suggest that two atoms may be held together by the electrical forces resulting from the transfer of an electron from one to the other. He assumed a shell of electrically positive matter within which there was a



static arrangement of electrons. Abegg[8] in an entirely independent paper published the same year, discussed the relation between electrons and ionization and the connection with older theories of Helmholtz and others. He also raises, I think for the first time, the question of polar and non-polar valences but seems to have decided that the former are more probable.[9] Rutherford[10] has advanced strong reasons for considering that atoms contain a positive nucleus around which electrons are rotating and this hypotheses has been further developed by Bohr,[11] Nicholson,[12] Moseley[13] and others.

Physicists in general have directed their attention to rotating or rapidly moving electrons and to the relation between these and spectral lines, the disintegration of atoms and other phenomena imvolving individual atoms. Chemists, on the other hand, following the suggestion of J. J. Thomson, have considered chiefly the rôle which the valence electrons probably play in the combination of atoms. Sir William Ramsay[14] in his address on "The Electron as an Element" considered that the electron takes a position between the two atoms which are held in combination. In a very recent paper, probably the last which he wrote,[15] he elaborates this thought further and describes models to illustrate the magnetic attractions which would result from electrons rotating in contiguous parts of two molecules. The magneton theory of the structure of the atom has also been developed elaborately by Parson.[16] It cannot account for ionization where, if we accept the electron theory at all, electrons must be transferred completely from the positive atom or group to the negative. Falk and Nelson,[17] Fry,[18] L. W. Jones,[19] Stieglitz[20] Bray and Branch,[21] G. N. Lewis,[22] and others have discussed the phenomena connected with the transfer of valence electrons from one atom to another but, with the exception of the magneton theories referred to above, no one, so far as I can discover, has suggested a possible connection between the motion of the valence electrons and chemical combination between atoms.

In the hypotheses here proposed the following assumptions, now more or less current among physicists and chemists, are made:

1. The atoms are of a complex structure made up of positive nuclei and electrons, of which the latter, at least, are in very rapid motion. If we assume that the electrons are 1/1800 the mass of hydrogen atoms and that they obey the same laws of motion as other atoms, their average velocity would be about sixty times the velocity of molecules of hydrogen (H_2). I will not attempt to discuss here the question whether the law of equipartition of energy actually holds for electrons.
2. That the electrons are of two kinds in their relation to the structure of the atom. Some of them are so involved in their orbits or motions among the positive nuclei that they can never escape from the atom. Others, called valence electrons, may be transferred to other atoms.

Let us suppose that two atoms, which have an affinity for each other are brought close together. A valence electron which is rotating around a positive nucleus in the first atom may find a positive nucleus in the second atom sufficiently close so that it will include the latter in its orbit and it may then continue to describe an orbit about the positive nuclei of the two atoms. During that portion of the orbit within the second atom that atom would become, on the whole, negative while the first atom would be positive. During the other part of the orbit each atom would be electrically neutral, and the atoms might fall apart. When we remember, however, the tremendous velocity of the electrons and the relatively sluggish motions of the atoms it seems evident that the motion of an electron in such an orbit might hold two atoms together. In ionization the electron would, of course, revolve about the nucleus of the negative atom leaving the other atom positive. It seems impossible to explain ionization otherwise than on the supposition of the complete transfer of the electron. This complete transfer in ionization is one of the strongest arguments against the magneton theory as the only explanation of chemical combination.

An interesting feature of the hypothesis proposed is that it may be used to account for that localization of the affinities in particular parts of atoms which is indicated by many of the properties of organic compounds. Thus if we suppose that there are four (or eight) positive nuclei in a carbon atom around which valence electrons may rotate, an atom of hydrogen may be held to the neighborhood of one of these nuclei as indicated in the figure.

I wish to acknowledge my indebtedness to Julius Stieglitz, R. D. Carmichael, J. B. Shaw, Jacob Kunz, A. P. Carman, A. A. Noyes and R. C. Tolman, who have read the first draft of this paper and of whom several have made helpful suggestions.

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[5] Noyes, W. A., *J. Amer. Chem. Soc., Easton, Pa.*, 23, 1901, (463).
[6] Stieglitz, *Ibid.*, 23, 1901, (797).
[7] Thomson, J. J., *Phil. Mag., London*, (Ser. 6), 7, 1904, (237).
[8] Abegg, *Zs. Anorg. Chem., Hamburg*, 39, 1904, (330).
[9] *Loc. cit.*, Note 8, p. 347.
[10] Rutherford, E., *Phil. Mag., London*, (Ser. 6), 21, 1911, (669).
[11] Bohr, N., *Ibid.*, 26, 1913, (1,476,857). On p. 862 Bohr discusses the hypothesis that atoms may be held in combination by electrons rotating about the line joining the positive nuclei of two atoms. This is similar to Ramsay's view mentioned below.
[12] Nicholson, *Phil. Mag., London*, (Ser. 6), 27, 1914, (54).
[13] Moseley, *Ibid.*, 26, 1913, (1024).
[14] Ramsay, Sir W., *London, J. Chem. Soc.*, 93, 1908, (775).
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[16] Parson, A magneton theory of the structure of the atom. *Washington, Smithsonian Inst., Misc. Collect.*, 65, 1915, No. 11.
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[18] Fry, *J. Amer. Chem. Soc., Easton, Pa.*, 34, 1912, (1268). *Zs physik. Chem., Leipzig*, 76, 1911, (385, 398, 591).
[19] Jones, L. W., *J. Amer. Chem., Easton, Pa.*, 36, 1914, (1268).
[20] Stieglitz, *Ibid.*, 36, 1914, (272); 38, 1916, (2046).
[21] Bray and Branch, *Ibid.*, 35, 1915, 1913, (1440).
[22] Lewis, G. N., *Ibid.*, 35, 1915, (1448).

Photographic Color Prints on Silk

The Manufacture Nationale des Gobelins has recently adopted the process of Messrs. Vallette and Feret for photographic color printing, the process being applied to articles de luxe that cannot well be printed by machine. Three successive impressions are made, in blue, yellow and red. The precision which is required in the superposition of the three impressions is secured by means of a special frame on which the article is stretched with the aid of metallic eyelets. The sensitizers used are alkaline phenols and diazo sulphites, and the colors are developed by exposure to the electric light.—*Engineering.*

The Use of Mean Sea-Level as the Datum for Elevations*

An Engineering Problem of National Importance

By E. Lester Jones, Superintendent, U. S. Coast and Geodetic Survey

The term "engineering" is usually considered to be synonymous with the word "efficiency," but in at least one branch of engineering it is only partly true. This is in leveling.

There is scarcely any surveying or civil engineering which does not require that differences in elevation be determined by spirit leveling, and in nearly all cases the absolute elevation of the bench marks above some plane of reference or datum is determined. Efficiency in operation and in the results frequently depends upon the datum selected.

What is a satisfactory datum? This is a very important question and one to which much thought is directed by engineers. If one is grading streets or extending a sewerage or water system in a city, it is evident that the official city datum should be used in the leveling operation; but if a city is to adopt a surface for its datum, what should it be? Evidently it should adopt that datum which is most generally used by State engineering departments, by the railroads which enter it, and by other engineering organizations.

Then the State itself must consider what datum it should adopt, and its decision should be based upon the previous decision of other States adjoining it.

Finally, what datum should the Nation adopt? It is evident that it would be impracticable for the Nation, especially one of 3,000,000 square miles of area, as is the case in the United States, to adopt the surface through some one bench mark as its datum in its early engineering work, for this would require that an elaborate system of levels should precede all its surveying and engineering operations.

Only slight consideration leads one to conclude that the ideal datum for a nation is one which may be established at many places. The only one of this kind is mean sea level.

Mean sea level is that surface which would be assumed by the surface of the water of the ocean if it were not disturbed by the attraction of the sun and moon and the force of the wind.

Mean sea level may be established within a very small fraction of a foot by continuous tidal observations for at least a year. It has been found from precise-leveling observations that mean sea level, as established at different points on the open coasts, is at all such points in the same equipotential surface; that is, if there were no resistence of the water and wind to the movement of an object floating on the ocean the object could be moved from one point on the coast to another without performing any work—there would be no lifting necessary.

While this statement may not be absolutely true, yet it is so nearly the case that for all engineering and surveying purposes it may be accepted as rigidly true.

If we have a surface at hand which makes an ideal datum for the elevations of the country, should it be adopted? Efficiency demands that it should be.

Mean sea level is now used exclusively as the datum for all surveys on land by the Coast and Geodetic Survey and by the United States Geological Survey. It is only occasionally used by the Engineering Corps of the Army and not always by the Reclamation Service.

Usually the General Land Office does no leveling in connection with its surveys of the public lands. There is no other Federal organization which covers large areas in its surveying work.

We can scarcely say, in consideration of the evidence, that mean sea level has been universally adopted by the Federal Government as the datum for all of its elevations.

In December, 1916, the Coast and Geodetic Survey sent the following letter, or one similar, to the chief engineers of most of the large cities of the country, to the State engineer of each State, and to the chief engineer of each of about 150 railroads in the United States:

> As you know, one of the important operations of the United States Coast and Geodetic Survey is the extension over the country of a network of precise leveling which will give elevations of great accuracy, based upon mean sea level.
>
> We believe that this precise leveling is essential in the surveying and engineering work done in this country by various public and private agencies. The network will enable engineers to use the sea-level datum on new projects and to reduce to this datum existing elevations referred to arbitrary datums. We believe that this country should eventually have but one datum, in order that all engineering and surveying work may be easily coördinated. We believe also that the presence of various datums leads to much confusion and waste.
>
> In order that we may get into touch with the needs of the engineering profession, I should be glad if you will let me know to what extent your State is basing the elevations of its road and other surveys and engineering works upon mean sea level; also whether the use of various arbitrary datums by countries, cities, and private organizations within your State is a serious matter in the industrial development of your State.

Replies were received from many of these engineers.

The engineers of most of the cities reported that arbitrary datums had been in use for many years. Many of them stated that it was generally realized that mean sea level should be used as the official datum, but that the expense incident to changing the many ordinances, profiles, plates, etc., made its adoption of doubtful expediency.

Responses were received from most of the State engineers and in all cases except one, they informed the Bureau that mean sea level was used as the datum for the State work wherever bench marks were available whose elevations were based on this datum. They strongly advocated the rapid extension of the precise-leveling system of the country in order that no extensive engineering and surveying should have to be based upon assumed elevations or arbitrary datums.

The chief engineers of many of the railroads replied to the inquiries of the Survey and, in nearly all cases, they stated that mean sea level was used, or was in process of adoption, by their roads. The general expression of opinion was that the precise-leveling net of the country should be completed, or greatly extended, in order that the accurate elevations might be at hand as the base for what might be termed the "detailed leveling."

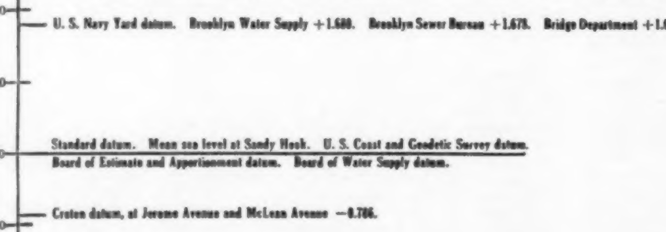
The chief engineer of the Topographical Survey Commission of the City of Baltimore wrote this office, in part, as follows:

> The city of Baltimore in 1893 established a series of precise-level bench marks which have been used since that time in connection with all engineering work carried on by the municipality. This survey has for its datum the mean low tide at Baltimore.
>
> The Pennsylvania Railroad, as well as other organizations, used still a different datum, so one can see that, although the city's precise-level work has been carried out to a degree of precision equal to that adopted by your survey, and is satisfactory for all city work, it would have been much better if the datum adopted for Baltimore had been that of mean sea level. This, I am sure, would have been done if at the time this survey was started a Government bench mark had been available. This would, to a certain extent, have done away with certain confusions which now exist.
>
> We believe that the plan for the United States Government to establish bench marks throughout the country, based on mean sea-level datum, would encourage the use of that datum by all who wish to carry on any extensive system of leveling, and would prove a great convenience and eliminate many errors and much confusion.

There is shown below the information given on a card issued by the municipal engineers of the city of New York. It was prepared by the special committee on datum planes. It gives a comparison of the various datum planes in use in the city of New York:

THE MUNICIPAL ENGINEERS OF THE CITY OF NEW YORK.

COMPARISON OF DATUM PLANES



For history of above datum planes see Proceedings of Municipal Engineers for 1915.—
Prepared by the Special Committee on Datum Planes November, 1916.

It is evident that in the city of New York there must be much confusion resulting from the use of so many datums, and it is possible that the confusion in the elevations and the datums may involve the city in much extra expense on engineering work, due to errors which must inevitably be made in interpreting the leveling data.

Conditions in the city of Cincinnati in regard to elevations were so unsatisfactory that, in August, 1913, the council of that city adopted by ordinance mean sea level as the official datum for all city surveying and engineering operations. Before the adoption of this datum each of the various sections of Cincinnati which had been an independent town had its own datum for levels, and there was no official connection of the various systems of elevations.

The city engineer of Birmingham informed the Coast and Geodetic Survey that mean sea level had been adopted as the official datum for that city. He said that Birmingham was made up of 13 municipalities, which were consolidated into one city government. At the time of the consolidation each of these municipalities had its own datum, and, owing to the fact that the city is about 14 miles long and 5 or 6 miles wide, it would have been a considerable expense to the city after the consolidation to undertake to harmonize all of these datum planes.

The city engineer of Memphis wrote, in part, as follows:

> I regret to advise that years ago an arbitrary datum was adopted here and that all of our city work is based on this datum. It has no relation and no meaning whatsoever. However, our records at the present time comprise probably 10,000 profiles and over 500,000 individual elevations, which would involve us in an enormous expense, with very serious chances of error, if we should try to change them. Personally, I should like to see them changed but I see no chance of getting an appropriation for the work, and if I did have it the job would be a staggering one.

The experience of the city of Memphis is what may be expected in any city that may adopt a datum not based on sea level. It is a warning to cities which have not yet adopted official datums that mean sea level should be the one selected.

That it is necessary to have precise leveling extended into areas where such control is not now available is clearly indicated by the data furnished by the city engineer of Salt Lake City. The only elevations available in Utah previous to about 15 years ago were from leveling by railroads, and that leveling was done for the immediate control of railroad construction rather than to carry absolute values with great accuracy. His letter contained the following:

> Below please find list of the different sea-level datum lines used in Salt Lake City. The city's datum being the lowest, I am calling it zero:

	Feet
Salt Lake City corporation	0.00
United States Weather Bureau	23.28
Oregon Short Line Railroad } United States Coast and Geodetic Survey }	26.95
Denver & Rio Grande Railroad	28.42

Frederick Law Olmstead, an authority on city planning, spoke of the need of a single datum, in his report of the Pittsburgh Civic Commission, which is entitled, "Pittsburgh—main thoroughfares and the downtown district," which appeared in December, 1910. In Part III, under the heading "Surveys and a city plan," he spoke of the need for surveys. One of the objects which he thought should be secured is an accurate framework of reference points, including (1) the gradual systematic setting of street monuments throughout the city to serve as reference points for the definite determination of street locations and for all public and private local surveys; (2) the accurate determination of the locations and elevations of these and other monuments and bench marks in reference to a single general system of coordinates and in reference to the United States Government bench marks; and (3) as a means of accomplishing these ends an accurate geodetic triangulation of the district, supplemented by the necessary precise traverse work and precise leveling all fully checked and compensated for errors.

It will be seen from the preceding quotations and statements that city engineers in general are in favor of mean sea level as the datum for their elevations, but many of them hesitate at present to advocate a change from the arbitrary datums on

*Special Publication No. 41, of the U. S. Coast and Geodetic Survey, Dept. of Commerce.

account of the expense involved. It is reasonably certain, however, that each city will eventually have to make a change, in order that the confusion incident to the presence of many datums in the city and in the contiguous territory may be eliminated.

The State engineer of Oregon wrote that in connection with the State's irrigation and highway work his department endeavored, wherever possible, to base its surveys on mean sea-level datum. He stated also that he found the bench marks of the United States Coast and Geodetic Survey and the United States Geological Survey of great benefit, as all surveys based upon them are on the same datum, and when connected form a uniform network over the State.

This is a very important point brought out by the State engineer of Oregon, for it is where new surveys must be connected with other surveys that great confusion arises unless all are based upon the single datum for elevations. If there were to be only one survey in a restricted area, and that survey would never have to be joined to any others, it would be a matter of indifference as to whether one datum or another were used for elevations.

The State engineer of Minnesota stated that he had not been able to use mean sea level throughout the State, but that as a general thing an arbitrary datum had been adopted in each county; and, in fact, in each of several counties more than one arbitrary datum had been adopted. The reason for this condition was the lack of bench marks in his State. He closed his letter with this statement:

We agree with you that it would be very valuable to the State if a system of levels could be established, and believe that such will need to be done in the near future in order to corelate the drainage, highway, and other engineering work in the State.

The deputy State engineer of New York informed this Office that the engineering department of that State has, since 1898, used mean sea level in all of its work. Previous to that time, elevations were upon a different datum for each of the three divisions of the New York State canals, those divisions having their headquarters at Albany, Syracuse, and Rochester. He said that several hundred engineers, who had been employed by his department, but were no longer connected with it on account of the completion of the barge canal, are now employed by municipalities and large corporations, or are in private practice. As they are familiar with the sea-level datum they are gradually changing the datums in different localities to that one. They are having considerable influence toward securing the general adoption of mean sea-level datum.

It might be well if the various States were to assist the Federal Bureaus in placing the leveling and other engineering data before the engineers of the States. This could be done very well by having the necessary data appear in the annual reports of the State engineers and of public utility commissions.

The various railroads of the country are vitally interested in the adoption of mean sea level as the sole datum for elevations in the country, for each of a number of railroads traverses the territory of several States and of innumerable cities and towns. There is necessarily a great deal of confusion in elevations between the railroads and the various municipalities and political units through which the roads pass if each unit has its own datum.

The Interstate Commerce Commission directed the various railroads of the country to show on their profiles the equations necessary to reduce elevations to sea-level datum or to have the profiles based upon mean sea level. This was for the valuation work undertaken by the commission. The result of this requirement has been that many railroads changed from arbitrary datums to the mean sea-level datum, although probably most of the roads of the country had previously adopted mean sea level as their datum. It may be interesting to note that the Board of Railway Commissioners of Canada has directed the railroads of the Dominion to submit all information in connection with elevations and profiles on mean sea-level datum.

The chief engineer of the Duluth & Iron Range Railroad Co. said:

Many other organizations in our vicinity use either an assumed datum or a datum which is supposed to correspond to the mean low water of Lake Superior. The discrepancy between the various datums is often confusing, and I have known cases where the confusion resulted in mistakes in construction which were expensive.

The chief engineer of the Philadelphia & Reading Railway Co. wrote in part:

Our system for some years back has used mean tide at Sandy Hook for its datum, and we believe that it is the only reasonable one for such purposes. In the city of Philadelphia, where an arbitrary datum is used, we have to make our plans conform to the Philadelphia city datum, in so far as such plans affect city improvements.

It is distinctly a serious matter to have different localities using different datums, and we shall be glad to see any movement toward the abolishment of any datums which are not based on mean sea level.

The chief engineer of the Western Maryland Railway Co. said:

We have had and still encounter trouble in avoiding errors due to the different datums used by adjacent railroads, various cities, countries, and private organizations. We are heartily in favor of the establishment of an authoritative datum for use and adoption by all.

The engineer of the Chicago, Peoria & St. Louis Railway Co. wrote that:

We are endeavoring to reduce the various arbitrary datums used in the construction of this line to sea-level datum. We do find that considerable inconvenience is occasioned by the use of arbitrary datums. Inasmuch as we are brought in touch with various drainage projects along the line we feel that some common datum should be used.

The chief engineer of the Missouri Pacific Railway wrote as follows:

It is very desirable, though not absolutely essential, to use one datum plane for all engineering elevations, as with conditions as at present existing there is a great deal of confusion and time lost in looking up proper equations to change from one datum plane to another.

In getting rough approximations of the discharge of streams it is necessary to know the approximate slope, and to get this it is necessary to know the elevations of crossings of the river at different points on its course. Where elevations of these crossings are referred to different datums it is frequently impossible to get more than a very rough approximation of the difference of elevations, which results in a corresponding approximation of the discharge. This also very frequently happens in connection with drainage work.

The use of various arbitrary datums by States, counties, cities, and private organizations is becoming more and more a serious problem, for the reason that it requires considerable research or investigation to determine whether the elevations used are referred to sea level or other datum planes and then to ascertain the correct equations.

I do not know of any one thing which the Coast Survey has undertaken which will be of greater benefit to the engineering profession of the country at large, in so far as all engineering operations are concerned—and by this one can almost say all industrial development of the country—than to promote and secure the adoption of mean sea level as the datum for all elevations.

The chief engineer of the Kansas City Southern Railway Co. said he believed that if a universal sea-level datum could be adopted by all State, county, and city organizations it would be of great benefit, not only to the railroads but to other industrial as well as private organizations. He heartily indorses any movement that would bring about this plan.

The chief engineer of the Chicago & Alton Railroad Co. stated that it is unfortunate that the municipalities have not, like the railroads, adopted the mean sea level. As an illustration he stated that almost every day situations occur in Chicago where elevations are referred to city datum; that is, the average elevation of Lake Michigan. This produces confusion, as it is necessary to consider the relation of the arbitrary datums with reference to mean sea level, which is almost universally used as the datum by railroads, geological surveys, etc. He added:

Any departure from the Government datum produces confusion and oftentimes embarrassing situations. I have had such experience in the city of Cleveland and in the city of Pittsburgh, where for many years no datum plane of any sort existed that was reliable and properly referenced.

The chief engineer of the Pennsylvania Railroad Co. informed the Survey that all construction on his road was based upon elevations on the sea-level datum, with the exception of some few cases where his work joined with that of cities, when it was necessary to have plans based upon the datums adopted by the municipalities. He expressed himself as being heartily in favor of reducing all elevations in the country to the mean sea-level datum.

The chief engineer of the Oregon Short Line Railroad Co. announced that his road is using mean sea-level datum for all of its elevations. He said that he found the results of the United States Coast and Geodetic Survey leveling invaluable for his use in connection with location and maintenance work. He expressed the belief that, if engineers generally could realize the saving in time and lessening in confusion which would result from the use of the mean sea-level datum, they would eliminate the various independent datums and adopt the standard sea-level datum.

The chief engineer of the Northern Pacific Railway Co. replied that there is a great deal of confusion at present caused by the various datums used for level work by different railroad companies, cities, and private organizations. He also said that the Northern Pacific is using sea-level datum, but that there is confusion caused by the fact that sea-level datum, as used by other organizations, had different elevations. He expressed the opinion that engineering work would be greatly facilitated if it could be based on the same datum for all the States.

This is another instance where elevations, supposedly based on mean sea level, are in error because of the lack of precise leveling elevations before the detailed leveling was run. This is an argument for the extension of the precise level net into the areas which now do not have this fundamental control.

The chief engineer of the Toledo & Ohio Central Railway Co. approved of any movement to establish reliable bench marks, referred to sea level, over the territory of the United States as, in his opinion, it would be of great value to the engineering profession and to all industries which have engineers in their service. He added:

In days gone by a great many surveys were made on our road and referred to any arbitrary datum that the man in charge might choose, with the result that we find it difficult and inconvenient many times to utilize properly the results obtained from these old surveys. All the work we are now doing is referred to sea-level datum.

The quotations contained herein from letters received from many of the most prominent engineers of the country prove conclusively, it is believed, that it would be far better for the country in its industrial and engineering developments if there were used only one datum and if that datum were mean sea level. It is realized by the members of the Coast and Geodetic Survey that much of the confusion in datums which now exists is due to the fact that the precise level net of the United States was not extended in the past as rapidly as it should have been. It, of course, was impossible, or rather impracticable, to extend a precise level net into areas through which railroads had not been run, for the expense would have been prohibitive. It may be that the Survey did not fully realize the necessity for having all engineering and surveying work on the same datum, but in recent years it has been fully alive to the necessity of having a single datum for the entire country, and it is consequently extending its precise leveling net as rapidly as funds available will permit.

To show how active the Survey has been in its precise leveling in recent years, it may be stated that in December, 1907, there were in the United States 24,000 miles of precise leveling which had been run by the United States Coast and Geodetic Survey, the Corps of Engineers United States Army, the United States Geological Survey, the United States Lake Survey, the Pennsylvania and the Baltimore & Ohio Railroads, together with the Mississippi and Missouri River Commissions. The amount of precise leveling in the country in December, 1916, was 35,500 miles. It may be interesting to note that during the calendar year 1916 about 2,500 miles of precise leveling were added to the net by the Coast and Geodetic Survey.

That the amount of precise leveling in the United States is entirely inadequate is shown in the following table which gives the amount of leveling in many of the countries of the world. This table also shows the amount of such leveling for each 100 square miles of area. We should double the amount of leveling in this country within the next few years.

Precise Leveling in a Number of Countries

Country	Area Square Miles	Miles of precise leveling	Miles of precise leveling per 100 square miles of area
United States	2,970,000	35,500	1.2
Alaska	591,000	20	.0
British Isles	121,000	12,804	10.6
Austria-Hungary	241,000	13,129	5.4
France	207,000	7,284	3.5
Germany	209,000	33,651	16.0
Italy	111,000	4,603	4.2
Japan	176,000	9,129	5.2
India	1,767,000	17,301	1.0

While it is of value to the nation for various organizations and individuals to adopt and use mean sea-level datum for their elevations, the country will benefit still more if each organization doing extensive leveling will publish in pamphlet form the elevations and descriptions of the bench marks they may establish in order that other organizations and individuals may properly coordinate their levels. Engineers are urged also to use substantial bench marks in order that future work may be benefited by their preservation. In this connection it may be interesting to quote from a letter which was sent to the Engineering Record by F. D. Yeaton, of Oak Harbor, Ill. This letter appeared in the issue of May 20, 1916, and is entitled: "Surveys and bench marks—more permanence needed." It reads as follows:

Sir: I would like to suggest a more general use of sea-level datum and reference ties to public land lines for all surveys, the lack of which will generally be found to be weak points in many surveys. The too frequent practice of using assumed bench marks, such as spikes in trees, or tops of curbs, should be discontinued wherever practicable. Reference points, such as stakes and marks opposite center of structures, are often used instead of township and section lines. It is only a question of a few years before assumed bench marks and reference points will be destroyed, and the surveys will be practically valueless for future use.

Recently I desired records of high-water marks along a certain strip of land to use in establishing a high-water plane. Several dozen surveys, which had been made for permanent bridge work and which contained high-water records, were obtained from the

files. As is often the case, the surveys had been made from assumed bench marks. The connecting links were missing and the surveys were valueless for my purpose.

One finds many maps, profiles, and topographical surveys on file in engineering offices bearing practically no direct relation to each other. Engineers have had occasion in valuation work, and in lawsuits involving surveys, to handle maps and profiles the value of which would have been intrinsic if only some relation between the maps and the profiles could have been established. The engineering departments of both small and large corporations are more or less at fault in the matter of allowing surveys to be made and filed which are of practically no future value.

The Federal Government has established sea-level bench marks and permanent land lines in many parts of the country, and their use should become more general in surveys of all kinds. The more general use of sea-level datum and land-line ties for surveys will aid greatly in reducing the duplication of field surveys, and will tend toward inagurating a more permanent and economical policy of making and filing surveys.

CONCLUSION

The Survey, whose obligation it is to extend the precise leveling net over the country for the use of Federal, State, city, and other engineering, surveying, and mapping work, feels that the expressions of opinion of the leading engineers of the country point to only one conclusion. This is that the Federal Government should provide the fundamental elevations in sufficient number that the various Federal engineering and surveying organizations and the smaller political units such as States, counties, and cities, as well as private organizations and individuals, may have available fundamental elevations upon which to start leveling, precise or otherwise, for detailed operations.

The amount of precise leveling which should be done by the Federal Government cannot be foretold. It must depend upon the needs of the various organizations and individuals using the results. After a certain development of the precise level net which appears now to be absolutely necessary, the rapidity with which further extensions are made should depend upon the development of the country. But such further extensions should precede rather than follow such development as is proved by the unfortunate condition of affairs in much of our engineering and surveying work, due to lack of precise elevations in the past, when such work was inaugurated.

With the increased demands for accurate topographic maps of the country, the wonderful extension of our good-roads movement, the extension of the railroads, and the development of river, irrigation, and drainage projects, the precise leveling of the country should be carried in the near future into many of the large areas now having little or no accurate leveling. The further conclusion may be drawn that it will be best for the efficiency of the country as a whole that all new leveling, by the various organizations and individuals engaged upon engineering and surveying work, be placed upon mean sea-level datum, wherever this is practicable, and also that as far as possible existing leveling referred to arbitrary datums should be changed to sea-level datum.

Meteorology and Aviation*

By W. H. Dines

A RECENT lecture by Lord Montagu of Beaulieu to the Aeronautical Society has directed attention to the possibility after the war of conveying mails and passengers, and perhaps goods, from place to place, by aeroplanes. In suitable weather such transit should present no difficulty save that of expense, provided that landing places can be found in such positions that the stages may not be too long, but it is obvious that the weather is, and must remain, a very important factor for many years to come.

The kinds of weather inimical to aviation are too much wind, low clouds and fog, and of these fog is perhaps the worst, as it is also in the case of shipping. The ways in which wind affects an aeroplane are various. There is the difficulty of starting and landing, but the days on which this is serious are not numerous, even in a windy country like England. But still there are days when lading is unsafe, and it is the misfortune with an aeroplane that it must in some way or other come to the earth as soon as its stock of petrol is exhausted. It cannot, like a ship outside a port with a dangerous bar, wait until conditions are more favorable; it must come down, whatever the risk. Once in the air, a steady wind has no effect upon the flying of an aeroplane, although it has a great effect upon the direction of the course. So much misapprehension exists on this point that it cannot be put too clearly. First, however, it must be stated frankly that a perfectly steady wind does not exist in practice, but the ordinary wind becomes more and more steady as the height increases, and in so far as the drift of an aeroplane is concerned it has the same effect as a steady wind of the same mean velocity.

The pilot, therefore, if the earth is hidden from him by a sheet of clouds, is absolutely and entirely ignorant of the strength and direction of the wind in which he is flying; it is just the same to him if it be a dead calm or if it be blowing at the rate of a hundred miles an hour from the east or from the west; he is, indeed, as unconscious of the motion which he is sharing with the air as he is of his daily revolution at a rate in these latitudes of some 600 miles an hour round the axis of the earth. But the effect upon the drift of his machine may be very considerable, and as he does not know what it is he cannot allow for it. The sailor is also concerned with the drift of his vessel, but he has in general a fairly good knowledge of how much it is; the currents due to the tide can be predicted, and the leeway due to wind can be estimated, but it is not so with the airman. Moreover, the rate of drift of a vessel is mostly small in comparison with her motion through the water, but in exceptional instances the velocity of the wind may equal the velocity of the aeroplane.

Thus Glasgow lies very close to a point 400 miles due north of Plymouth, and an aeroplane leaving Plymouth and flying due north at eighty miles an hour would find itself close to Glasgow in five hours' time. Should, however, a strong west wind be blowing of which the pilot did not know, and also clouds so that he could not see the earth, he would, if steering by compass, find himself in five hours' time over the North Sea, and quite possibly much nearer to the Danish than to the English coast. In the present state of our knowledge he could obtain information at starting of the general direction and strength of the wind, but not in such detail that he could hit off Glasgow within 100 or 200 miles. If he could see the ground he could ascertain that he was not traveling in the way his machine was pointing, and would thus become aware of his drift, but if he could see the ground he could steer by the known landmarks. There would be few landmarks over the sea, but the appearance of the surface should give him information as to the strength of the wind, and also of its direction.

Hence it seems likely that in countries like England, where clouds prevail, long-distance flight, if it is to be carried on at regular times day after day, will have to be at low elevations. About 3,000 feet is the usual height of the winter cloud sheet, but it may on occasion easily descend to 2,000 feet.

Wind, therefore, though when it is steady and in a favorable direction it may be of assistance for a journey in its own direction, will in general be a hindrance to aerial navigation, and when combined with low clouds may become an insuperable hindrance. In cases where its velocity and direction can be accurately foretold, the difficulty about allowing for the drift can be overcome, but such precise forecast is not yet practicable.

A gusty wind introduces difficulties of its own; the so-called holes in the air, of which one heard so much in the early days of aviation, were due to gustiness, but greater stability and speed in the machine are eliminating these difficulties.

Clouds introduce a difficulty of their own, apart from the point that has been already considered. It would seem at first sight as though a man would retain his sense of the vertical direction in any circumstances, but this is not so. Were a man placed inside a hollow vessel that was falling freely without air resistence, he would be entirely without sense of weight or direction, and the pilot of an aeroplane in an extensive mass of cloud is in much the same position. He cannot see any definite object, and apart from sight his sense of direction depends upon the reaction between him and the seat he is sitting on. So long as the motion is uniform this reaction is vertical, but any acceleration of the machine alters the direction and intensity of the reaction, and so confuses the sense of level. The same effect is produced upon a spirit-level or similar instrument, and so confusing is the effect that it is said the machine may almost be upside down without the pilot knowing it. It would seem as though a gyroscope might to some extent meet the difficulty. One result of this uncertainty of level is that astronomical observations for the determination of latitude and longitude are not possible unless the horizon can be seen, and thus the amount of the error produced by want of knowledge of the drift cannot be known.

Fog is to all intents and purposes simply a cloud touching the earth. Landing places for aviators have naturally been put in low, sheltered positions, partly because a shelter from wind is required, but probably because more or less of a dead level is necessary, and such flat places are more likely to be found at low altitudes. Such positions are especially liable to fog. The danger of a fog lies in its concealing the landing place and hiding from the pilot until the last moment his distance from the ground.

Thus it appears that the demand of the airman on the meteorologist is that he shall be able to forecast wind and fog, and, to a less extent, clouds, on the route the airman is proposing to follow. It has long been the business of the Meteorological Office to forecast wind, and a certain amount of precision has been attained.

During last winter Major Taylor investigated the pos-sibility of forecasting fog, and gave the results in lectur to the Royal Meteorological and Aeronautical Societie His work constitites a considerable advance in vestigation of this difficult subject. If we express th wind in terms of its two components, W. to E. and S. t N., the probable error of a forecast for each compon is perhaps about ten miles an hour, and there is not mu prospect of improving this; the estimate is for Engla and the Continent, but farther south the conditions ar much better.

I do not wish to emphasize the difficulties which lie i the way of regular air services, but they are there, an the first step towards overcoming them is to admit thei existence.

*From *Nature*.

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